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DELAYED LOSS OF FUNCTION DUE TO BRAIN LESION¹

AUSTIN C. HERSCHBERGER

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Two hypotheses have been advanced to account for restitution of function after brain lesions. Von Monakow (1914) has advanced the argument that any cortical insult results in a kind of shock-like reaction which extends far out from the site of the lesion, incapacitating many functions which may clearly be unrelated to those of the immediately damaged tissue. If this be so, then the variety of symptomology which might arise from similar lesions is to be expected, dependent upon the extent of the shock.

The second hypothesis to be advanced was that of Alford (1948). He observed the numerous instances of negative findings with regard to aphasia: lesions having occurred without the supposedly appropriate symptoms; symptoms having occurred without expected accompanying lesions. He was also impressed by the generalized psychological instability of patients after cortical insult—functions disappearing, reappearing and again disappearing sporadically. He further observed several instances where gross lowering of visual function occurred although the optic radiations near the site of the disturbance were not directly involved. In these instances the half-field of the retina showed that the gradient of threshold excitability was raised, yet the relative sensitivity of the half-field from the periphery to the central limit remained intact. This latter observation led him to the conclusion that von Monakow's concept of diaschisis was inadequate to account for this variable loss of function. He argued that, instead of a shock-like disruption accompanying every brain insult, there was an edema-like reaction which spreads not only to tissue adjacent to the lesion but also to more remote loci in the brain. This "distance reaction" has the property of radically depressing, but not entirely incapacitating, the activity of the neurones concerned. He bases his argument for this depressive quality upon his observations of the visual excitability cited above.

Forgays (1952a) has attempted to measure the recovery cycle in both humans and animals. He found no differences in scores on any

¹ Based on a dissertation submitted to the faculty of the Department of Psychology of the University of Illinois in partial fulfillment of the requirements for the Ph. D. degree. The author is indebted to Professor L. I. O'Kelly who directed the investigation.

of the psychometric tests used when testing humans before and immediately after operation. By the third postoperative day there was a significant decrement in the Fourth Word Series, the McGill Picture Anomaly Series, the Stanford Binet Vocabulary Test, and the Wechsler Digit Span Test. These discrepancies subsequently recovered to the postoperative level by the 20th day.

Using rats, Forgays (1952b) produced relatively small lesions (5.2 per cent of the cortex for each pair of lesions) bilaterally in four distinct areas of the cortex; frontal, parietal, occipital, and cerebellar. The small lesions, relatively little loss of blood, and short duration of anesthesia enabled him to test his animals within an hour after completion of the operation. He found that the error scores on closed mazes of the operated animals were not significantly different from sham controls within the first four hours. At this time the parietal group began to make significantly more errors than the other three groups. The parietal group remained defective for two weeks. The remaining groups, frontal, occipital, and cerebellar, showed decreasing frequency of errors in that order during the same period, although the differences were not significant. These studies lend supporting evidence for Alford's position as against that of von Monakow's diaschisis.

Results of a preliminary study by the present investigator tended, although not significantly, to confirm Forgay's results (1952b). The period of greatest deficiency was found to be around three weeks, about the time that Forgay's animals showed complete recovery. The problems used in these investigations, however, are not comparable. Forgays used what was essentially a transfer situation while this investigation used a new learning situation, a 14 unit multiple T maze. Because of the impossibility of differentiating between the position of Alford and that of von Monakow when using a problem which requires incremental learning over a period of time, a pilot study was undertaken, the results of which indicated that an avoidance response, which could be learned within a period of an hour, might be useful in accurately describing the course of events during recovery from brain lesions. A large scale experiment was then conducted which, it was hoped, would more accurately describe the phenomena.

METHOD

Subjects: 120 naive female rats between 90 and 120 days of age from the Sprague-Dawley strain maintained in the laboratory colony at the University of Illinois Psychology Department were used in this experiment. They were divided into eight equal groups of 15 each; normals, sham-operates; etherized; one-hour operates; one-day operates; one-week operates; three-week operates; and six-week operates.

Differential treatment of groups. Surgical procedures for operates

were as follows: Animals were placed under moderate ether anesthesia and the hair from a level somewhat behind the ears to the level of the anterior aspect of the eyes was clipped with scissors. The skull was then laid bare by a midline incision extending from a level of the ears to the posterior aspect of the eyes. Underlying tissue was teased away from the skull bone. The skull was trephined bilaterally, using a No. 5 dental trephine, approximately one millimeter posterior to the juncture of the frontal, squamosal and parietal sutures. The meninges in the operates was then removed and the underlying cortical tissue removed by aspiration. Only the tissue underlying the trephined hole was removed. The operation lasted from fifteen to twenty minutes. After aspiration of the tissue, the wound was closed with 22 millimeter Michel wound clips and then painted with a 10 percent solution of iodine. Finally the wound was painted with a solution of collodion to prevent the animal from scratching it open.

The shams were treated similarly to the point of removing the meninges and tested after a one-hour rest. The etherized group was placed under moderate etherization for 20 minutes in order to duplicate the etherization treatment received by the operates and allowed to rest one hour subsequent to being placed in the problem situation. The one-hour operates were allowed one hour's rest before being placed in the avoidance apparatus. The one-day group was tested 24 hours after the operation had been terminated. The four other delay groups were returned to their home cages where they were maintained on the standard animal room routine, being taken from their cages on the appropriate testing day.

Apparatus. The apparatus in which the animals were trained was a hurdle-type avoidance box. The inside dimensions of the box were 23 inches long, 6 inches wide, and 8½ inches high. The bottom of the box was formed by a grid of stainless steel rods 3/32 inches in diameter and spaced at intervals of 3/6 inches. The circuitry was so arranged that the two halves of the grid could be charged independently. An aluminum barrier was placed between the two halves of the box which extended 2 inches above the grid floor. The barrier was spring loaded which allowed the rat easy passage, yet kept the barrier in place between the halves.

During the resting period the box was illuminated by three 7½ watt frosted bulbs mounted just above the glass ceiling of the box. They were operated during this period through a resistance of 650 ohms. The shock was operated at 200 volts through a resistance of 150,000 ohms in series with the grid. The light stimulus was accomplished by removing the 650 ohm resistance from the illuminating circuit and interrupting the current three times per second through an electronic interrupter. The onset of the light stimulus was controlled by a manually-operated spring-loaded toggle switch. This switch also

set in motion an automatic timing device which, after a five second delay, charged the grid preselected by the experimenter. The switch also set in motion a Standard Electric 0.01 second timer, which was inactivated with the release of the toggle switch. All equipment which did not provide constant auditory stimulation, such as timers, relays and the interrupter, was housed in sound deadened boxes.

Learning Procedure. The animal was taken from its home cage and placed in the apparatus for a 10-minute adaptation period. After this lapse of time, the first conditioned stimulus of the blinking light was presented for five seconds. The grid upon which the animal was resting was then electrified until the animal crossed the barrier. Both light and shock stimuli were terminated at this point. This procedure was repeated every thirty seconds after the onset of the previous light stimulus, i.e., every time the second hand of the stop watch passed either the 60 second mark or the 30 second mark. This was continued until the animal successfully anticipated the shock by jumping the barrier before onset of shock for ten successive trials without error. If by 270 trials the animal had not met the criteria of ten perfect trials in succession, the animal was removed and returned to its home cage. Its score for the series was counted as 270 trials. When an entire group had finished training, they were sacrificed and the brains removed. The brains were placed in a 10 percent solution of formalin. Mapping and measuring the extent of the lesion was determined by drawing the surface lesion on Fortuyn diagrams. The area of the lesion was then determined from the diagrams with a Willis planimeter and expressed as a percentage of the total cortex.

RESULTS

Brain Damage. The average damage for all animals was 5.5 percent of the total cortex. The location of the lesions was in the occipito-parietal areas of the cortex. Analysis of variance, F ratio of 1.39, indicated that the differences in area of lesions among the various groups were not statistically significant.

Avoidance Learning. Preliminary investigation indicated that many if not all the animals in the one-hour delay group could be anticipated to fail to learn within the 270 trial limit. Accordingly, two non-parametric statistical tests were chosen beforehand to analyze the results; the H -test as described by Walker and Lev (1953) for over-all differences that might exist among the groups; and the Mann-Whitney U test for differences between various groups. The H -test for over-all differences among the various groups yielded a value of 454.87. At six degrees of freedom a value of 18.5 or larger is required at the .001 level of significance, indicating that there were highly significant differences among at least some of the groups. The values obtained in Computing U are listed in Table 1.

TABLE 1

TABLE OF MANN-WHITNEY U VALUES FOR COMPARISONS
BETWEEN VARIOUS GROUPS IN AVOIDANCE LEARNING

Groups	U Value	Significance Level
Normal and Etherized	35 190	.001
Etherized and Sham	180.5 116.5	Not Significant
Sham and One-hour operates	32 193	.001
Etherized and One-hour operates	26 199	.001
Normal and Sham	35.5 188.5	.001
One-hour and One-day operates	34 193	.001
One-day and One-week operates	96 126	Not Significant
One-week and Three-week operates	76 149	.10
Six-week and Three-week operates	9.5 215.5	.001
One-day and Three-week operates	60.5 164.5	.02
One-day and Six-week operates	39.5 185.5	.001
One-hour and Three-week operates	48 177	.005
Normal and Six-week operates	103.5 121.5	Not Significant

TABLE 2

THE MEDIAN SCORE FOR EACH GROUP IN AVOIDANCE
LEARNING

Group	Median trials to criterion
Normal	44
Etherized	97
Sham	72
One-hour operates	270
One-day operates	98
One-week operates	116
Three-week operates	200
Six-week operates	47

Another striking comparison can be found by an inspection of the median scores of the various groups which are found in Table 2.

Both hypotheses were substantiated. Shock due to brain insult is clearly evidenced by the inferior performances of the one-hour operates to all other groups. The presence of a spreading "distance reaction" is evidenced in the difference between the one-day and the three-week operates, and its diminishing effect by the difference between the three-week and the six-week operates. Not anticipated, but of interest is the difference between etherized non-operates and normals.

DISCUSSION

Two processes have been demonstrated to be operative as a result of brain insult. Von Monakow's notion of some kind of shock-like reaction is clearly demonstrated in the decidedly inferior performance of the one-hour operates. His idea that the shock gradually subsides does not, however, seem to be borne out. Although the one-day group did not return to a level equal to that of normals or the six-week operates, they did return to a level of performance comparable to that of the sham and the etherized group. It is at this point that a flaw in the design is revealed. Although it would seem unlikely that the effect of ether would be so prolonged as to interfere with performance 24 hours later, one cannot categorically attribute the performance of the one-day group to the effect of the operation alone. Had the effects of ether been anticipated, a group of etherized animals with a 24 hour delay would have been included to clarify this particular point.

Alford's hypothesis that there is something occurring within the brain tissue which temporarily makes it increasingly more difficult for brain injured subjects to perform adequately was borne out by the difference between the one-day operates and the three-week operates. The difference between the three-week and six-week operates indicated that this disruptive factor had subsided.

Neither "vicarious functioning" nor "spontaneous neural reorganization" seem adequate explanatory concepts which would account for the cycle of events found in this study. "Vicarious functioning" does not seem to apply in as much as the response, albeit there in a diminished form, was still available to the animal one day after the operation. Prolonged training after an operation and then ablation of another site with subsequent disappearance of the function in question is required for the assumption of "vicarious functioning." Likewise, "neural reorganization" would hardly be responsible, for were it so, the process during reorganization would temporarily inhibit rather than facilitate the response: witness the inferior quality of the three-week operates in comparison to the one-day and six-week operates. The more parsimonious explanation appears to be neither one of "vicarious function"

nor one of "neural reorganization" but rather that the neural tissue ordinarily mediating this response was temporarily inhibited.

Hicks and Warren (1950), in studying the cytological sequence in brain repair from injuries, describe the following order of occurrences:

"Within two or three days mononuclear phagocytes began to appear in the injured region. This acute stage of phagocytosis is maximal during the first week and subsides slowly thereafter . . . As the activity of this acute stage begins to subside another secondary process develops, becoming conspicuous about the third week. This is the proliferation of astrocytes, the matrix cells of the brain, around the edge of the infarct and laying down of their fibers—gliosis."

There is evidence, then, that at least two physical processes are occurring after brain damage which seem to overlap each other and which parallel somewhat the behavioral findings stated above. It could well be that the metabolic by-products of these processes are intimately related to the "edema-like" substance which Alford feels lowers neural function in surrounding tissue, and until this edema has subsided, behavioral reactions will either be suppressed or will not appear.

SUMMARY

1. 120 naive female rats between 90 and 120 days of age were subjected to various intervals of rest after operation and subsequently tested for ease of acquisition of an avoidance response.

2. The results indicate that both a shock-like effect occurs immediately after brain injury and that a suppressive effect occurs about three-weeks later.

REFERENCES

- ALFORD, L. B., Cerebral localization, outline of a revision, *Nerv. and Ment. Dis. Monogr.*, 1948, 77, 3-99.
- FORGAYS, DONALD G., Reversible disturbances of function in man following cortical insult, *J. comp. physiol. Psychol.*, 1952. (a)
- FORGAYS, DONALD G., Reversible disturbances of function in rats following cortical insult, *J. comp. physiol. Psychol.*, 1952. (b)
- HICKS, S. P., and WARREN, S., *Introduction to Neuropathology*, McGraw-Hill Book Company, Inc., New York, Toronto, London, 1950.
- MONAKOW, C. von, *Die Lokalisation im Grosshirn und der Abbau der Funktion durch kortikale Herde*, Bergmann, Wiesbaden, 1914.
- WALKER, HELEN M., and LEV, J., *Statistical Inference*, Henry Holt and Company, New York, 1953.

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SOME PERSONALITY VARIABLES ASSOCIATED WITH BINOCULAR RIVALRY

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The stereoscope has a long history, and has been extensively employed to study various psychophysical effects such as binocular rivalry, three-dimensional space perception, color interaction, and other visual phenomena. Only recently, however, has an attempt been made to investigate, by means of a modified stereoscopic technique, some aspects of vision and perception usually more closely identified with psychology as such. For example, Engel (1955) found that a learning set influences binocular rivalry when meaningful content is introduced. A few other preliminary investigations (Adlerstein, 1958; Davis 1956) suggest that such a stereoscopic technique might have some application to the study of personality and perception, particularly the study of perceptual defense or facilitation.

In the usual operation of the stereoscope, two similar stimuli are simultaneously and separately exposed, one to each eye. However, if the stereoscope is modified somewhat so that entirely different stimuli are exposed to each eye, the stimuli then will be seen as merged, as alternating, or with one predominating and the other suppressed (Engel, 1955).

Previous studies in the area of perceptual defense, although very controversial, suggests that positively valued stimuli tend to enhance or facilitate perceptual recognition, and that negatively valued and affectively charged stimuli tend to produce an inhibitory effect (Allport, 1955). It might be expected, therefore, that if stimuli such as short words were shown in the stereoscope, and were so arranged that an emotional word were presented to one eye, and a less emotional word to the other, perceptual recognition of the emotional material would either be facilitated or inhibited, depending upon the defense mechanisms of the person. We shall refer to subjects who tend to report more emotional than nonemotional words as "facilitators." Subjects who tend to suppress emotional words we shall refer to as "inhibitors."

¹ From the Research Laboratory, Veterans Administration Hospital, Palo Alto, California. Credit is due to Drs. George Krieger and Robert Mowry of the Palo Alto Veterans Administration Hospital, and to their staff, for administrative help and for rating their patients. The author is also indebted to Miss Beverly Collins, research technician, and to Mr. Arnold Goldman, research psychology trainee, for their valuable technical assistance.

In the present study the prediction was made that subjects who tend to see emotional words involving the areas of aggression, sex, and dependency in the stereoscope (i.e., are "facilitators,") will more freely express fantasy themes involving these areas than subjects who tend to see less emotional words in the stereoscope. It was also predicted that the facilitators, because of their heightened ability for emotional expression, would report themselves as more hostile and aggressive, and would be judged as more aggressive in their overt behavior.

METHOD

Apparatus. The stereoscopic task was similar to the one described by Engel (1955), and consisted of a Keystone stereoscope enclosed by a cardboard box. The box was partially open at the top so that the slides could be inserted, but could not inadvertently be seen by the S. The stimulus words were typewritten on cardboard slides and illuminated by two small lights in each of the lower corners of the stereoscope.

Subjects. Approximately one hundred patients in the Palo Alto Veterans Hospital were selected on the basis of cooperation and a maximum age of 45, and were screened for eye dominance (position preference). A cutting point of 75% eye dominance was employed, so that subjects showing a position preference over 75% of the time were discarded. Twenty-four patients survived the screening, and were included in the experimental group. The age range was from 20 to 45, with an average of 33.2 years.

Procedure. In addition to the stereoscopic task, a "Hostility Questionnaire," and ten cards of the Thematic Apperception Test (TAT) were administered. A few weeks after testing, each patient was rated on hostility by his psychiatrist, nurse, and psychologist.

a. For the stereoscopic task, 30 stimulus words were employed, including ten each for the areas of Sex, Dependency, and Aggression. The stimulus words were paired with less emotionally-toned words, approximately matched for frequency on the Thorndyke-Lorge word lists (1944). For example, some pairs for the sex categories were: "kiss-keep," "naked-novel." Dependency pairs were "devotion-dispatch," "receive-realize." Aggressive pairs included: "explosion-expansion," "kill-keep." The number of letters of the words in each pair was the same, and each began with the same letter of the alphabet. In order to compensate for a position set, the paired words were alternated, so that each word was visible once to each eye. A practice list consisting of neutral words was presented prior to the emotional lists. The number of emotional words reported was scored, so that high scores were intended to represent facilitation, and low scores inhibition. Reported words were defined as

the word first seen, or the word predominantly seen. Usually the word first seen predominated.

b. Following the stereoscopic task, a 75 item questionnaire designed to measure self-reports of aggression was administered. This has been referred to as the "Hostility Questionnaire," and was developed by Buss and Kurkee (1957).

c. Ten cards of the standard TAT series were then presented in the usual manner, and scored by two raters on the basis of expressions of sexual, dependency, and aggressive content. These cards were 1, 3BM, 4, 6BM, 7BM, 8BM, 13MF, 14, 17BM, and 18BM.

d. Two or three weeks after the previously described tests had been administered, the patients were rated on a five point scale by their ward psychiatrists, therapists, and nurses, and consensual ratings were obtained for aggressive behavior in the hospital.

RESULTS AND DISCUSSION

The results indicated that patients who demonstrated a facilitation for conflict or emotional words seen in the stereoscope involving the areas of aggression, sex, and dependency, tended to express more fantasies involving these conflictual areas in TAT stories than individuals less responsive to emotional words. It will be seen in Table 1 that the correlations between aggressive words seen in the stereoscope and aggressive themes was .34. Similarly, for sex and dependency the correlations were .17 and .54 respectively. The correlation for the stereoscopically presented total word list and all three TAT categories was .37. The low correlations for the sex category may be explained on the basis that the TAT pictures were selected primarily for their ability to elicit aggressive themes, so that relatively few sexual themes

TABLE 1

PEARSONIAN CORRELATIONS BETWEEN STEREOSCOPICALLY PRESENTED EMOTIONAL WORDS AND TAT THEMES

Stereoscopic words	TAT scores			
	Aggression	Sex	Dependency	Total
Aggression	.34 **			
Sex		.17		
Dependency			.54 *	
Total				.37 **

Note: * Significant at better than the 1% level (one-tailed test)

** Significant at the 5% level (one-tailed test)

were expressed. With the exception of the sex category, the correlations were significant.

The results also support the prediction that individuals who tend to report more emotional words in the stereoscopic task (facilitators) also tend to be more aggressive throughout different areas of behavior.

Table 2 shows the correlations between the total stereoscopic scores and the Hostility Questionnaire, the TAT, and ward ratings. The Hostility Questionnaire was designed to tap the area of reported aggression. From Table 2 it can be seen that these admissions of hostility correlated .47 with the total stereoscopic scores, which was significant at the .01 level with a one-tailed test. The next highest correlation was between the TAT measures of aggressive fantasy and the stereoscopic scores, and correlated .38, significant at the .05 level. The lowest correlation was between the stereoscopic scores and ward ratings of aggression, and yielded a correlation of .31 which just misses significance at the .05 level.

TABLE 2

PEARSONIAN CORRELATIONS BETWEEN PERCEPTUAL FACILITATION AND EXPRESSIONS OF AGGRESSION DERIVED FROM A HOSTILITY QUESTIONNAIRE, TAT, AND WARD RATINGS. N=24.

Hostility Questionnaire	TAT	Ward Ratings
.47 *	.38 **	.31

Note: * Significant at the 1% level (one-tailed test)

** Significant at the 5% level (one-tailed test)

SUMMARY AND CONCLUSIONS

A stereoscopic technique involving the pairing of emotional with less emotional words was developed in order to explore the relationship between binocular perception of emotional words with some personality variables in a psychiatric population.

The following conclusions seem justified:

1. Perceptual defense and its personality correlates can be studied by a stereoscopic technique employing binocular rivalry.
2. The results indicated that hospitalized patients who demonstrate a facilitation for, or a sensitivity to, conflict or emotional words in the stereoscope involving the areas of aggression, sex, and dependency, tended to show more fantasies involving these conflicts in TAT stories than persons less responsive to emotional words.

3. Patients who show a facilitation for emotional words reported themselves more hostile in a questionnaire and showed more aggressive fantasies in the TAT than patients less responsive to emotional words. There were also some suggestions that patients demonstrating facilitation for emotional words tended to be rated as more overtly aggressive, but these results just missed significance.

REFERENCES

- ADLERSTEIN, A. M. The humanscope: A modified stereoscope. *J. Psychol.*, 1958, 45, 109-113.
- ALLPORT, F. H., *Theories of perception and the concept of structure*. New York: Wiley, 1955.
- BUSS, A. H., AND KURKEE, A. An inventory for assessing different kinds of hostility. *J. consult. Psychol.*, 1957, 21, 343-349.
- DAVIS, J. Four studies in personality. Unpublished senior thesis, Princeton Univ., 1956.
- ENGEL, E. The role of content in binocular resolution. *Amer. J. Psychol.*, 1956, 69, 87-91.
- THORNDYKE, E. L., AND LORGE, I. *The teacher's word book of 30,000 words*. New York: Bureau of Publications, Teachers College, Columbia Univ., 1944.

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THE EFFECT OF ALCOHOL UPON REACTION TIME IN THE WHITE RAT

WILLIAM C. STEBBINS, ROBERT W. LUNDIN
and DAVID O. LYON

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The development of reaction time involves a combination of stimulus discrimination and response differentiation. At the human level, instruction in the conventional reaction time experiment typically requires a rapid response or sequence of responses immediately following the onset of a previously specified stimulus or stimulus complex. Training, with the possible exception of a few practice trials, is unnecessary due to the long established and extensive repertoire of both verbal and manual behavior on the part of the subject. For the infra-human subject, on the other hand, prolonged discrimination training under the relevant reinforcement contingencies is necessary as a substitute for verbal instructions. Skinner (1938) has described such a contingency as a "double discrimination". In the present investigation, a response in the presence of a given discriminative stimulus is reinforced only if it occurs within a critical period of time.

When responding under one stimulus on a given schedule produces a second stimulus in the presence of which a response is reinforced, the schedule is called a chained schedule (Ferster and Skinner 1957). Specifically, under the conditions of the present study, the experimental organism is required to wait, i.e. not to respond for a certain time interval under one stimulus condition. When this contingency has been fulfilled, a second stimulus, a light, is presented and the animal's first response is reinforced but only if that response has occurred within a fixed critical period of 3 sec. After the critical period, the stimulus conditions are again changed and the sequence is repeated. When a stable behavioral output has been obtained under these conditions, an independent variable, in this case alcohol, can be manipulated in a systematic fashion.

The only other experimental investigation relevant to this procedure was performed by Sidman (1955). Rats were trained on a *drl* schedule for water reinforcement. The reinforcement was contingent upon a 21 sec. period of no responding. That is, Ss were reinforced only if one response of bar pressing followed another by at least 21 sec. When rats were conditioned according to this method for 30 to 60 hours, a good time discrimination developed. Most of the responses followed each other by 18 to 21 sec. With intraperitoneal injections of a 10% solution of ethyl alcohol, there was a 50% decline in response rate. Although the alcohol

produced a general depression in the response rate, it had relatively little effect on the time discrimination.

The purpose of the present study was to investigate the effect of alcohol upon the latency of a discriminative operant in the white rat. After an animal's reaction time to a light stimulus had become stabilized through selective reinforcement, the effect of alcohol could be measured.

METHOD

Subjects

Four experimentally naive, male albino rats of the Wistar strain were used. They were approximately 100 days old at the beginning of the experiment. Following each experimental session, they were fed 15 gms. of Purina laboratory chow.

Apparatus

A modified Gerbrands rat box with Gerbrands bar and Anger rotary pellet dispenser were installed in an ice chest which was fitted with an air blower. The box was situated in a sound deadened room equipped with forced air ventilation. The light and food magazine were controlled and the behavior recorded with magnetic counters and associated relay circuitry. Reinforcements consisted of .05 gm. Noyes pellets. Chronoscopes were used to record the reaction times.

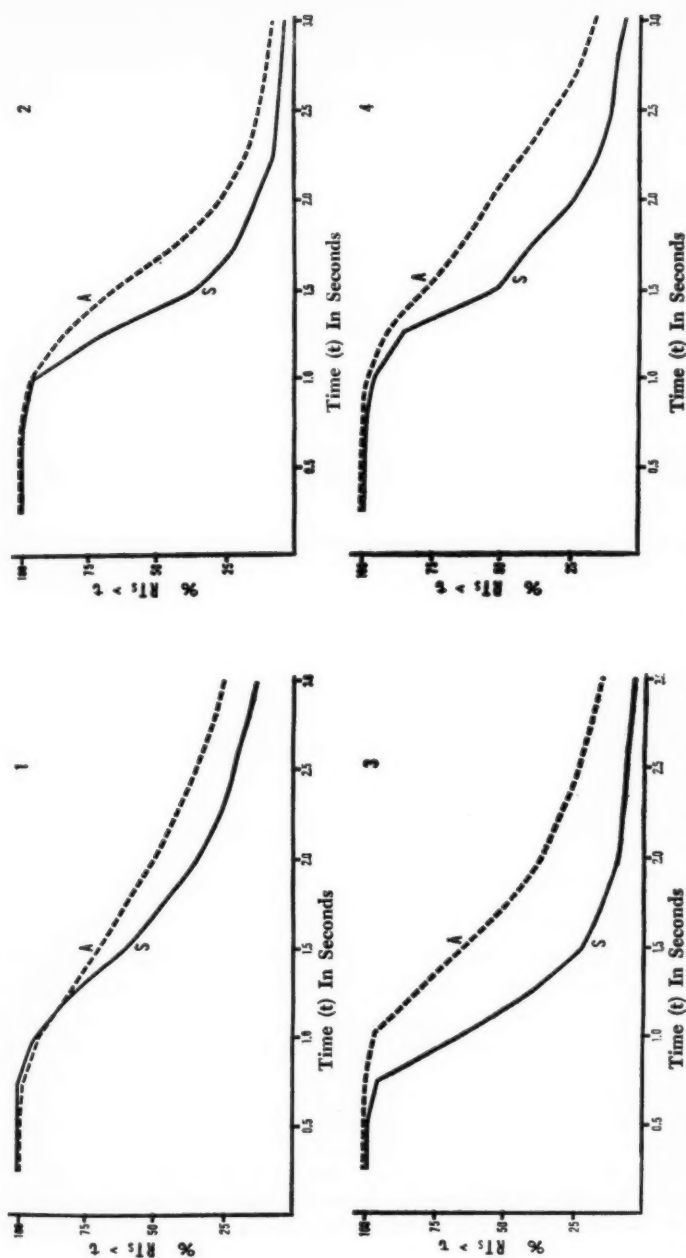
Procedure

Following conditioning with 150 regular reinforcements, the animals were given discrimination training for 2 hour sessions on alternate days. A small neon pilot light (S^D) was presented after a 30 sec. period of no responding in the dark (S^Δ). The first response following the light onset was reinforced after which the light was immediately terminated. When this discrimination was stabilized, the reaction time training was begun by reducing the critical duration of the light to 3 sec. Only responses during this critical 3 sec. period were reinforced. Any response with a latency greater than 3 sec. would turn off the light and no reinforcement would be forthcoming.

After the reaction time became stable at about $1\frac{1}{2}$ sec. the rats were given intraperitoneal injections of 3 ml. physiological saline solution. On alternate days Ss received injections of ethyl alcohol solution ranging from .4 ml. of a 10% ethyl alcohol solution to 3 ml. of a 20% ethyl alcohol solution. For two rats the maximal dosage under which responding could be maintained was 2.4 ml. of a 20% ethyl alcohol solution and for the other two the dosage was 3 ml. of a 20% ethyl alcohol solution. Injections were administered immediately prior to each experimental session.

RESULTS AND DISCUSSION

Figures 1-4 show the cumulative percent frequency distributions of reaction times for each animal with values ranging from .25 to 3 sec.



Figs. 1-4. Cumulative percent frequency distributions of reaction times for each of the four animals under conditions of alcohol (A) and saline (S). Any point on the curves represents the percentage of reaction times greater than that time on the abscissa. The functions were obtained from the final sessions for each of the two conditions.

The size of the class interval is .25 sec. These data were obtained from the final experimental session in which the maximal dosage of alcohol was used and from the final session for saline. The functions are presented for each individual subject. For rats 1 and 2 (Figs. 1 and 2) the maximal dosage of alcohol was 2.4 ml. For rats 3 and 4 (Figs. 3 and 4) the maximal dosage, represented in the curves, was 3 ml. The differences between alcohol and saline are least marked for animal 1 (Fig. 1). However, beyond the point at which the curves cross, the difference between the two functions is significant ($\chi^2=10.98$ $p<.01$). For the three other animals (Figs. 2, 3, and 4) the differences between the alcohol and saline functions are significant beyond the .001 level ($\chi^2>13.83$; $p<.001$). In computing the χ^2 , the Kolmogorov-Smirnov one tailed test was used (Siegel 1956). This non-parametric statistic was chosen because the reaction time distributions were skewed rather than normal. The data for each of the individual animals clearly indicate significant increases in reaction time under conditions of alcohol over those for the saline controls. The same trend can be illustrated by inspection of Table 1 which shows the median reaction times for each of the four animals under conditions of alcohol and saline. In all cases the median reaction times for alcohol were greater.

TABLE 1
MEDIAN REACTION TIMES (IN SECONDS) FOR THE FINAL
ALCOHOL AND SALINE SESSIONS

Rat	1	2	3	4
Saline	1.69	1.39	1.08	1.52
Alcohol	2.00	1.64	1.69	2.06

TABLE 2
TOTAL NUMBER OF RESPONSES FOR THE FINAL ALCOHOL
AND SALINE SESSIONS

Rat	1	2	3	4
Saline	227	211	304	265
Alcohol	211	185	114	246

Table 2 indicates the effect of alcohol on the total number of responses during the final session in which alcohol was administered. With the exception of animal 3, the effect of alcohol was to produce only a slight decline in responding. The two dosages of 20% ethyl alcohol solution produced only a minimal reduction in the total number of responses compared with the 50% decline reported by Sidman (1955) with lower dosage levels. Although animal 3 showed a 63% drop in response rate with alcohol, the median decline in total output for all Ss was only 9.5%. On the other hand, the effect of alcohol on reaction time

was more pronounced; the median increase in reaction time for the 4 Ss was 27%.

SUMMARY

Four male albino rats of the Wistar strain served as Ss in an exploratory experiment designed to study the effect of alcohol on the reaction time of a discriminative operant. After the animals had acquired a light-dark discrimination, reaction time training was begun by setting up a criterion of 3 sec. following the onset of a light. Any response after this time interval went unreinforced and the light was terminated for 30 sec. After the reaction times were stabilized at about 1½ sec., the Ss were injected with a saline solution alternating with injections of ethyl alcohol.

The results show that this technique is suitable for measuring the effect of alcohol upon reaction time in the white rat. Intraperitoneal injections of 2.4 ml. and 3 ml. of a 20% ethyl alcohol solution significantly increased the reaction times for all animals. However, the administration of alcohol produced only a slight decline in lever pressing output (total number of responses).

REFERENCES

- FERSTER, C. B. AND SKINNER, B. F. *Schedules of reinforcement*. New York: Appleton-Century-Crofts, Inc. 1957.
- SIDMAN, M. Technique for assessing the effects of drugs on timing behavior, *Science*, 1955, 122, 925.
- SIEGEL, S. *Nonparametric statistics for the behavioral sciences*, New York: McGraw-Hill Book Co., 1956.
- SKINNER, B. F. *The behavior of organisms*, New York: Appleton-Century Co., 1938.

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DRIVE ACCOMMODATION AND LEARNING¹AARON J. BROWNSTEIN AND W. A. HILLIX²*University of Missouri*

When drive is induced by some daily deprivation period, it is usually assumed that drive level remains constant after some threshold number of days. This assumption is often necessary because manipulated variables may vary together with number of days on the deprivation schedule. Thus, if drive were not constant, the relationship between the behavior in question and the manipulated variables might be obscured.

Recently two attempts to test this assumption have been reported. Hall, Smith, Schnitzer, & Handford (1953) showed that the rat's daily drum activity reached an asymptotic level after 12 days on a 23-hr. deprivation schedule. Reid and Finger (1955) reported that body weight, food and water intake, and 24-hr. drum activity, all showed progressive changes for at least 15 days on a 23-hr. deprivation schedule. Drum activity in the hour preceding feeding progressively increased throughout the 35 days of the experiment.

If any of the above measures are related to the dependent variable, a confounding would be expected between drive accommodation effects and the effects of manipulated variables in those experiments in which less than at least 15 days of accommodation to the deprivation schedule were given. Since less than 15 days of accommodation seems to be typical, this is certainly in need of experimental testing.

The non-equivalence of measures found in Reid and Finger's experiment indicates that quite different results might be obtained in other situations. The present paper reports an experiment in which the effects of accommodation on *reinforced* responding in a straight alley were studied.

METHOD

Subjects

Ss were 20 naive male albino rats, approximately 90 days old at the start of the experiment. An additional 12 animals were discarded for reasons given below.

¹ This investigation was supported by a research grant, M-817, from the National Institute of Mental Health, National Institutes of Health, Bethesda, Maryland.

² Now at Navy Electronics Laboratory, San Diego, California.

Apparatus

A straight alley nine feet long, three inches wide, and five inches deep served as the apparatus. Guillotine doors twelve inches from each end of the alley partitioned the alley into starting box, runway, and goal box. The goal box was flat black, the rest of the apparatus gray. The entire apparatus was covered by $\frac{1}{2}$ -in. hardware cloth. Starting time and total time were recorded by electric clocks activated by the doors, and running time was obtained by subtraction.

Procedure

Five days of accommodation to the experimental room and individual living cages were given. Ss were then given five days of alley adaptation, during which time they were allowed to explore the alley in groups of four; food was available throughout the alley and goal box during this 30-min. period. Ss were then divided into four groups equated for weight, and placed on a 22-hr. deprivation schedule. Group A15 Ss were deprived of food on the night of the last adaptation period. Groups A10, A5, and A1 were deprived subsequently, so that the groups were on schedule for 15, 10, 5 and 1 day preceding the first test night.

Feeding time was staggered so that each animal could be tested at his own feeding time; this was felt to be necessary in case factors like activity just prior to feeding might affect straight alley performance. Order of running was counterbalanced groupwise to equate any effects of time of night on running behavior.

Five massed test trials were given per night for five nights. Five trials were given in order to provide a fairly reliable measure of performance for each group on the critical first test night. Reward was two little Frisky pellets (.45 Mg.). If an S failed to enter the goal box within three minutes, he was taken out and discarded from the analyses reported. Two Ss in A1, two Ss in A5, and one S in A10 were discarded for this reason. Therefore, the one S in A10 and two Ss in A15 with the highest single trial total time were discarded to eliminate any possible selectivity factors that might otherwise result from the discarding. The illness of another S in A5 necessitated the random discarding of one S from each of the remaining groups. The resultant N for group was five.

After all training was completed on the last test night, ten massed extinction trials were given before the animals were fed. The same procedure and measures used during training were used, except that animals that did not enter the goal box within two minutes were taken out and started again.

RESULTS

An analysis of variance on first test night weights revealed

significant differences among groups, although A5 Ss were heaviest and A15 Ss lightest.

Analyses were done on log running time, log total time, and log 1/starting time. A Lindquist (1953) Type VI analysis was used. The log running time analysis revealed a significant difference among groups ($F=3.73$, $p<.05$ for 3/16 df). The mean log running time for Groups A1 through A15 were .804, .527, .545, and .551, respectively. This difference, although significant at only the .05 level, was quite stable, with Group A1 remaining clearly slower over all five training days. No differences were found between groups on either the log total time or log 1/starting time analyses ($F=1.87$, $F=1.28$, for 3/16 df).

Analyses of the above measures for the ten extinction trials revealed no significant group differences.

If a confounding effect of drive accommodation were present, as the activity studies suggest, a significant difference between groups on the first test night would be expected. A second difference would be expected in the analysis of the changes in group performance from day to day over the five day training period.

An analysis of variance on log running time for the first test night revealed no differences between groups. Similarly the groups by days interaction effect was insignificant for log running time, log total time, and log 1/starting time ($F=1.24$, $F=1.49$, $F=1.24$, for 12/64 df), although the days analysis was significant in every case, indicating that Ss performance did not reach an asymptote too early to prevent the occurrence of an interaction.

DISCUSSION

The results of this study indicate that five days of accommodation to a 22-hr. deprivation schedule is sufficient to establish a stable level of performance in a straight alley. This finding is at variance with the results of activity studies (Hall et al., 1953; Reid & Finger, 1955) reporting changes in response level for approximately 15 days on a similar deprivation schedule.

Certain operational differences may account for the disparate findings. Our dependent variable was based on a rewarded response, theirs were not. Our deprivation schedule was calculated to avoid differential weight losses; their schedule was more severe. Their measures were taken from the beginning of deprivation and were based on progressive changes within a single group; our measures were taken only after a given level of accommodation had been reached.

Another possible explanation may lay in the role of the drive

stimuli (Hull, 1943) in the eliciting stimulus pattern. It should be noted that in both activity studies referred to, care was taken to minimize the effects of environmental conditions. It is conceivable that under these conditions drive stimuli constitute a significant portion of the eliciting stimulus pattern, whereas the greater number of stimulus elements in the straight alley should minimize the role of the drive stimuli. A similar hypothesis regarding the role of drive stimuli has been advanced by Webb (1955).

Thus it is contended that the energizing effects of the traditional method of deprivation (Verplanck, 1953) may become stabilized more quickly than the stimulus effects. Stability of performance level beyond the threshold of the energizing effect becomes then a function of the role of the drive stimuli in the eliciting stimulus pattern.

SUMMARY

Four groups of rats under 22-hr. deprivation were given five acquisition trials per night for five nights in a straight alley. The groups differed in the number of days that they had been on the deprivation schedule prior to the first training night. No evidence for an effect of days on schedule was found beyond the fifth day.

The disparity between these results and those of activity studies is discussed in terms of the operational differences in procedure and in terms of the possible stimulus products of drive.

REFERENCES

- HALL, J. F., SMITH, K., SCHNITZER, S. B., and HARDFORD, P. V. Elevation of activity level following transition from ad libitum to restricted feeding. *J. comp. physiol. Psychol.*, 1953, 46, 429-433.
- HULL, C. L. *Principles of behavior*. New Haven: Yale Univer. Press, 1943.
- LINDQUIST, E. F. *Design and analysis of experiments in psychology and education*. Boston: Houghton Mifflin, 1953.
- REID, L. S., and FINGER, F. W. The rat's adjustment to 23-hour deprivation cycles. *J. comp. physiol. Psychol.*, 1955, 48, 110-113.
- VERPLANCK, W. S., and HAYES, J. R. Eating and drinking as a function of maintenance schedule. *J. comp. physiol. Psychol.*, 1953, 46, 327-333.
- WEBB, W. B. Drive stimuli as cues. *Psychol. Reports*, 1955, 1, 297-298.

The Psychological Record, 1960, 10, 25-27.

AUDITORY SCANNING IN THE DOLPHIN¹

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The purpose of this report is to present an analysis of a unique behavior pattern of the bottle-nose dolphin or porpoise, *Tursiops truncatus* (Montagu), as it locates objects by echo ranging in water. This marine mammal—which is actually one of the smaller of the toothed whales—is known to avoid obstacles in its pathway while swimming and to locate fishes for food by sound reflection or by echolocation (Kellogg, 1958; 1959a). To accomplish such a result, it sends out trains or series of short staccato sound pulses, the echoes from which are reflected back to the animal's ears (Kellogg, Kohler, & Morris, 1953). Porpoises have been shown experimentally to be able to discriminate between food-fishes of different size and to select one fish from another by this method after visual cues have been effectively eliminated (Kellogg, 1959b). The man-made or mechanical analogue of porpoise echo ranging in water is represented by Navy sonar or by the marine fathometer.

Locating a Small Target

In swimming toward a small target such as food-fish held beneath the water, a dolphin can often be seen to move its head and trunk alternately to the right and left. Movements of this nature have been reported in a single captive specimen by Schevill and Lawrence (1956) and have been described in two additional animals by Kellogg (1958, 1959a).

Oscillating head movements occur only (1) while the animal is emitting trains of sound pulses and (2) when the target toward which it is swimming cannot be visually identified because of the turbidity of the water. Observations of the phenomenon indicate that a complete cycle takes place in from 2 to 3 sec. This activity has been noted at distances up to 10 ft. from the target and it continues until the dolphin gets close enough to seize or take the bait.

Moving the target slowly in a direction perpendicular to the animal's line of approach causes him to turn continuously so as to keep the target in the median plane. There is no interruption in the angular oscillations under such conditions. If the target is moved rapidly, there is an appreciable and clearly observable latency in the orienting behavior which

¹ Contribution No. 110 from the Oceanographic Institute of Florida State University. The observations reported in this paper constitute a part of a longer program of research supported by the National Science Foundation (grants No. G920 and G1730). Some of the underwater acoustical equipment used to listen to and record porpoise sonar signals was loaned by the Office of Naval Research (contract Nos. Nonr 531 and Nonr 1502). The porpoises were donated by the Marine Studios of Marineland, Florida.

results. The aim of the porpoise lags behind the actual position of the target at any instant. If the direction of movement of the target is abruptly reversed, the animal will overshoot the turning point (Kellogg, 1959b).

The speed of forward motion in such cases has been calculated for one animal and has been found to be quite slow. In 172 measurements made over a period of 4 weeks it averaged .34 ft./sec. with a standard deviation of .06 ft./sec. and a range of .44 ft./sec. (The average of .34 ft./sec. is roughly equivalent to .2 miles per hr.) The magnitude of the arc of oscillation of the head and body is estimated at approximately 10 degrees, or 5 degrees on either side of the median plane. What takes place is diagrammed schematically in Fig. 1.

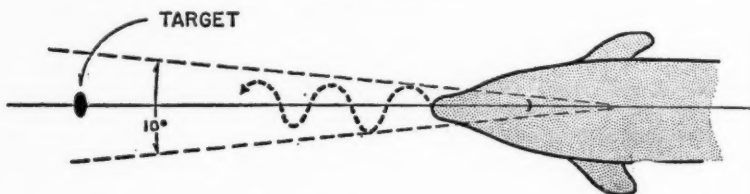


Figure 1. Auditory scanning in the bottlenose dolphin. When echolocating a small target, such as fish, the porpoise approaches by oscillating its head to the right and left through an arc of about 10 degrees. This behavior undoubtedly involves binaural localization as well as echolocation.

Significance of the Behavior

The only adequate interpretation of this activity appears to be that the animal is orienting to the echoes of its own sound signals which are reflected back from the target it is approaching. The original sound-pulses broadcast by the dolphin are not highly directional—probably no more so than the human voice. But the echoes returning from an object at a fixed point in space would vary continuously as the locus of the animal's ears changed with reference to the source of the echoes.

The oscillation of the head is exactly what one would expect in the pin-pointing of an object by means of reflected sound. It is the same sort of activity in which a human being would engage if he were employing binaural localization. The head movements of the dolphin would constantly modulate the phase and intensity differences of the echoing sound waves reaching each of the ears. They would also alter the time interval between the emission of a pulse-signal and the return of its echo to each ear. The continuation of the process as the animal swam forward would enable him to determine with great precision the direction from which the echoes came.

Auditory Scanning

Since the noises which make up the echoes are emitted by the dolphin itself, the activity as a whole amounts to a kind of scanning by sound. We suggest the term *auditory scanning*, therefore, as a good

name for both the acoustic and the general behavior comprising this elaborate pattern of activity. The parallel with optics is enhanced if auditory scanning is compared to visual scanning which is conducted at night with the aid of a searchlight.

From the standpoint of the organism, auditory scanning may be thought of as more complex than visual scanning, for it represents a combination of two already complex processes. Auditory scanning consists of (1) the emission of a continuous series of sound-signals for the purpose of echolocation and (2) binaural localization. It might be characterized as "binaural-echo-localization."

Above and beyond this activity is the perceptual process itself. The stream of information produced by auditory scanning must be instantly analyzed by the amazing brain of the animal. Greatly enlarged cerebral hemispheres, particularly in the temporal region, give an anatomical clue to the remarkable acoustical ability which the dolphin possesses. The mechanical counterpart of such a receptor system would be a sonar apparatus with one transmitter and two independent receivers, plus an electronic computer capable of decoding and processing the data—all within a single compact unit.

SUMMARY

As the bottle-nose porpoise swims toward an underwater target it frequently oscillates its head from side to side through an arc of about 10 degrees, at the same time emitting trains of sound signals. The phenomenon is accounted for as a combination of echolocation and binaural localization. It is given the name of auditory scanning.

REFERENCES

- KELLOGG, W. N. Echo ranging in the porpoise. *Science*, 1958, 128, 982-988.
- KELLOGG, W. N. Auditory perception of submerged objects by porpoises. *J. Acoust. Soc. Amer.*, 1959, 31, 1-6. (a)
- KELLOGG, W. N. Size-discrimination by reflected sound in a bottlenose dolphin. *J. comp. physiol. Psychol.*, 1959, 52, 509-514. (b)
- KELLOGG, W. N., KOHLER, ROBERT, and MORRIS, H. N. Porpoise sounds as sonar signals. *Science*, 1953, 117, 239-243.
- SCHEVILL, W. E., and LAWRENCE, B. Food-finding by a captive porpoise (*Tursiops truncatus*). *Breviora (Mus. Comp. Zool., Harvard)*, 1956, 53, 1-15.

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IS THE "CLICK" A SECONDARY REINFORCER?

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Bugelski (1956) points out that in recent years the role assigned to secondary reinforcement has become very powerful "... so powerful, in fact, that it has virtually replaced primary reinforcement." Myers (1958) complains that although secondary reinforcement has been widely regarded as a panacea for reinforcement theories, it is inadequately defined, inadequately demonstrated, and that there is much disagreement about its relation to a number of variables and many gaps in our knowledge.

According to Deese (1952) a reinforcement is any stimulus which can increase the strength of a response when it is placed in close temporal conjunction with that response. Since our paper requires a distinction between primary and secondary reinforcement, the term primary reinforcers will be used to refer to a class of stimuli which, without special training in the experimental situation serve as reinforcers according to the definition given by Deese. According to general usage, secondary reinforcers are previously neutral stimuli which acquire reinforcement value, that is, can strengthen a response when placed in close temporal conjunction with that response, by virtue of their association with other reinforcers.

The two types of measures which have been used in secondary reinforcement experiments are the learning of a new response and resistance to extinction. Typical of the learning of a new response situation is Miller's acquired drive experiment (Miller, 1948). Miller seems to have demonstrated satisfactorily that fear may be evoked by previously neutral stimuli, and that in the presence of such stimuli a new response may be learned. The role of the black box as a learned reward, or a secondary reinforcer, is much less conclusive. It is likely that if a box of any other color, shape or size had been substituted for the black safety box, the new response would have been learned just as readily. As in most experimental situations, the choice of behaviors available to the S was limited. To escape from the white box, Miller's S's had to go to the black box. That the black box had acquired new functional properties is debatable.

An experiment by Bugelski (1938) illustrates the resistance to extinction studies which are used as support for the secondary reinforcement concept. He gave two groups of rats identical training in which the response was closely followed by a click and food. One group was extinguished with the click present, and the other with the click absent. The click present group made significantly more responses to extinction. Although such evidence seems to indicate that the click, a previously neutral stimulus, served to maintain the response, there is no evidence that the response was strengthened by the presence of the click during extinction. The observed extinction effects can be explained by the hypothesis that the number of responses to an extinction criterion vary directly with the number of stimuli present during extinction which were also present during acquisition. Bitterman et al (1953) and Mackintosh (1955a) have reported experiments which support the hypothesis that differential extinction effects are a function of the similarity between conditions of training and extinction. Such a hypothesis seems more adequate than one based on secondary reinforcement, since no evidence of response strengthening is present.

If the secondary reinforcement label is appropriate for such previously neutral stimuli, it seems reasonable to assume that their effects should be demonstrable during acquisition as well as during extinction. Apart from studies such as Miller's (1948) where the previously neutral stimulus is presumed to affect the learning of a new response, few investigators have attempted to investigate the validity of the secondary reinforcement concept with reference to acquisition. Although Melching (1954) did vary the percentage of neutral stimulus presentations during acquisition, he reported only extinction results. In spite of the lack of evidence concerning the effect of previously neutral stimuli on acquisition, the secondary reinforcement concept has been used frequently to account for differences in acquisition. The present study is designed to investigate whether such previously neutral stimuli do serve as secondary reinforcers during acquisition.

According to Hull (1952) and other continuity theorists, each additional reinforcement adds an increment to response strength until an asymptote has been reached. If this is so, then on trials in which the response is contiguous with a stimulus which has secondary reinforcing value, an increment should be added to the response strength. It is possible that this increment is less than if the response were associated with a so-called primary reinforcer, but the increment should certainly be greater than if the response were associated with neither a primary nor a secondary reinforcer.

METHOD

In the preliminary phase of the experiment, a click was paired

with food prior to the introduction of acquisition training. During acquisition, the number of food plus click trials was held constant, while the number of click alone trials was varied. If the click served as a secondary reinforcer, it would follow that the strength of the response during acquisition would increase with the proportion of trials on groups.

Table 1 presents a summary of the experimental design. The numbers refer to acquisition trials. The click was introduced as the previously neutral brief stimulus. Response strength was measured in terms of acquisition latencies. For Groups 1, 2, and 3 the frequency of the previously neutral stimulus was varied systematically. Primary reinforcement in the form of food reward was held constant for these which the click was present.

TABLE 1
EXPERIMENTAL DESIGN

Preliminary Training	Exper. Groups	Acquisition Conditions			Extinction
		Food and Click	Click Alone	No Food or Click	
All Ss treated identically:					Half the Ss from each group extinguished
Habituation;	1	24	24	0	
food and click	2	24	12	12	with click
paired 40 times;	3	24	0	24	present and
food, click and					half with
bar paired 10	4	48	0	0	click absent.
times.					

Extinction training was included as a control measure. It was recognized that a number of potential "secondary reinforcing agents," such as visual stimuli, were present in the apparatus in addition to the click. It therefore appeared essential to demonstrate that the click had acquired effects supervening those of the other stimuli present in the situation.

The experimental design permitted the following specific hypotheses:

1. At the end of acquisition training, the latency of the bar-pressing response should increase from Group 1 to Group 3, since the number of trials during which the click was present decreased from Group 1 to Group 3.
2. Subjects undergoing extinction with the click present

will make more responses to an extinction criterion than subjects extinguished without the click.

Group 4 was included to permit the following comparisons:

1. The effect of total vs. partial food reward on the latency of the response during acquisition.
2. The effect of total vs. partial food reward during acquisition on responses to extinction.

Subject and Apparatus

The Ss were 48 albino rats from 90 to 150 days in age from the colonies maintained at the University of Oklahoma. The apparatus consisted of a box divided into a starting compartment, 6 x 12 in., and a goal compartment, 18 x 12 in., a retractable bar, and an automatic recorder of the bar press latency. The *E* could manipulate the concomitance of the pellet of food and the click with the bar pressing response.

Experimental Procedure

For 4 days prior to and throughout the experiment all Ss received a reduced diet of 8 gms. of Purina chow daily. During all experimental sessions Ss were under 22 hrs. food deprivation. All Ss were given two days of habituation training, each day consisting of 10 trials in which S was placed in the apparatus, where *E* released a pellet in the food box. The S remained in the apparatus for one minute after the release of the pellet. On the following two days each S was given 20 trials per day in which *E* released the pellet and sounded the click simultaneously. These trials were designed to establish a relationship between the click and the pellet. The bar was not present during any of these initial trials, but was present thereafter. On each of the next two days each S was given five bar pressing trials, the pressing response being accompanied by both pellet and click.

At the completion of the preliminary training, Ss were assigned randomly to one of the four acquisition groups indicated in Table 1. Groups 1, 2 and 3 received food and click on 24 of the 48 trials. The pattern of food trials was predetermined randomly and identical for these groups. Presentation of the click on non-food trials varied. Group 1 received the click on all 24 non-food trials, Groups 2 and 3 on 12 and 0 of the non-food trials respectively. Group 4 received the food and click on all 48 trials. Twelve trials per day were given for 4 days. The interval between trials was one minute.

Extinction training began the day following the completion of acquisition training. Each acquisition group was subdivided randomly into two extinction sub-groups, one of which was extinguished with the click present, the other with the click absent. Each S was given 20 minutes of free responding, and the number of bar-presses was recorded.

RESULTS

The basic data for the test of the first specific hypothesis were the latencies of the bar press responses for the four days of acquisition training. The score for each S was derived by obtaining the median of his 12 acquisition latencies, for each of the 4 days. Table 2 summarizes these data, with the group medians being the medians of the individual S medians. When these data were analyzed by the Kruskal-Wallis one-way analysis of variance none of the differences was significant. Therefore, there is no evidence that the different acquisition treatments affected the acquisition latency scores.

TABLE 2
MEDIAN LATENCY IN SECONDS OF THE BAR-PRESS
RESPONSE FOR EACH OF THE FOUR ACQUISITION DAYS

Group	Day 1 Median	Day 2 Median	Day 3 Median	Day 4 Median
1	4.80	3.05	1.80	1.30
2	5.20	1.75	1.10	1.15
3	5.60	2.60	1.85	1.25
4	3.45	2.55	1.60	1.65

TABLE 3
MEAN NUMBER OF BAR-PRESS RESPONSES
DURING EXTINCTION

Acquisition Groups	Extinction Sub-group Click - Present		Extinction Sub-group Click - Absent		Click - Present plus Click - Absent	
	Mean	S.D.	Mean	S.D.	Mean	S.D.
1	51.50	8.25	44.33	17.44	47.92	14.11
2	51.83	15.62	37.67	15.52	44.75	17.12
3	61.12	21.79	52.67	18.49	56.92	20.66
4	37.50	18.49	25.00	5.48	31.25	15.00

Table 3 presents the means and S.D.'s for bar press responses during extinction. Application of analysis of variance to these data indicated that the difference between the click present and click absent extinction sub-groups was significant beyond the 5 per cent level. The difference between acquisition groups was significant beyond the 1 per cent level. When the *t* test was used to determine the source of the differences between groups in terms of the differential acquisition treatments, it was found that Group 4 differed significantly from Groups 1 and 3, while the difference between Group 4 and Group 2

approached significance. The partial reinforcement groups (Groups 1, 2 and 3) did not differ significantly from one another.

The results of the statistical analyses permit the following conclusions: 1. Variations in the frequency of the click during acquisition did not affect response latency during and at the end of acquisition. 2. Variation in the frequency of the click during acquisition did not affect the number of responses during extinction. 3. The presence of the click during extinction resulted in more extinction responses. 4. Response acquisition proceeds as rapidly with partial food reward as with total food reward. 5. Partial food reward during acquisition results in more responses to extinction than total food reward does.

DISCUSSION

The results of this experiment would seem to permit two paradoxical conclusions. First, it would seem that the click during acquisition does not serve as a secondary reinforcer. Groups 1, 2 and 3 did not differ significantly in response latency during or at the end of acquisition, nor did they differ in number of extinction responses. However, the presence of the click during extinction did affect the number of extinction responses. Such data have been interpreted by others as evidence that the click serves as a secondary reinforcer.

Since it seems unlikely that the click should change roles from acquisition to extinction, alternative explanations should be sought. It could be that differences between acquisition conditions were too slight to permit possible differences in response strength as measured by latency to become apparent. The well known variability of latency as a response strength measure lends plausibility to this suggestion. A repetition of the acquisition phase of this experiment with a much larger number of trials, permitting greater variation in click presentation, and preferably with a response measure in addition to latency, would seem worth while. The extinction training was added merely as a control measure, and as such, was very brief. Had extinction been continued until a rigorous criterion was met, it is unlikely that the click present-click absent results would have been changed, since these results are in agreement with those of other investigators (e.g. Bugelski, 1938). There is, however, a slight possibility, that had the extinction training been more complete, a difference between Groups 1, 2, and 3 might have been discerned during extinction. This possibility would seem worthy of future investigation.

Since data are not yet available to evaluate the possible methodological factors, an attempt must be made to interpret the data at hand. Why should the click seem to serve as a secondary reinforcer during extinction but not during acquisition? Indeed, why label the click as a secondary reinforcer at all? Why not merely consider it as a stimulus

which has been associated in a specifiable manner with both food, and the bar pressing response, without adding the secondary reinforcer label?

Estes (1958) proposes that the probability of a response at any given time is equal to the proportion of stimuli which are available that are conditioned to that response, and cites evidence to support his hypothesis. If his hypothesis is valid, then it would seem that the click should be considered as one of numerous other stimuli in the acquisition situation, and at that, not one which should have too great an effect on the bar pressing response. The click occurred after the S pressed the bar and rather than acting as an elicitor of the bar pressing response, would seem to act as an elicitor of the food-cup approach or eating response. An experiment by White (1953) reviewed by Bugelski (1956) seems to substantiate this point. White employed an enlarged Skinner box designed to "explode" the behavior pattern and thus enable the counting of both lever pressing and "food-cup approach responses." White found that rats receiving "pre-extinction" of the "foodcup approach response," accomplished by presenting a click (previously associated with the reception of food) while the bar was absent, made both fewer bar depressions and food-cup approaches during a subsequent extinction session with the bar present than did control subjects. On the basis of this finding, Bugelski concludes that the click was a conditioned stimulus for the "food-cup approach response" and was not in any sense a secondary reinforcing agent for the bar-press response.

Had our experimental situation been such that the click could serve as a signal that a bar pressing response at that time would be followed by food, the results might have been different. However, in this experiment, as in most of the the so-called secondary reinforcement experiments, S had no way of "knowing" whether the response would be followed by food, click or neither. Consequently, the groups, including Group 4 which received food and the click after every bar pressing response, showed no differences in response latency. Indeed, why should they? All Ss had been deprived of food for a sufficient time to warrant the assumption that they were hungry. Food was obtainable by bar pressing alone, and no cues were available to indicate that a particular bar pressing response would not be rewarded. It seems safe to predict that great variations in response latency could have been produced by varying the presentations of the click as a signal that the bar pressing response would be followed by food.

Theoretically, a reinforcer is supposed to add an increment in response strength. There is no evidence from the acquisition phase of this experiment that the click performed such a function. However, the lack of difference during acquisition between Group 4 (total food reward) and the other groups (partial food reward) does not indicate

that even each food reward added an increment to response strength. The lack of difference is corroborated by the results of Mackintosh (1955b) and Sheffield (1949). Actually few investigators have recorded such acquisition data, since most of the interest has been directed toward the effects of differential reinforcement schedules on extinction. The determination of the effects during acquisition of interspersing non-food trials requires further experimental study. One possibility is that the interspersing of non-rewarded trials causes frustration. Frustration might serve as a drive which increases the performance of the responses, without necessarily affecting learning as such. A recent experiment by Adelman and Maatsch (1956) suggests that under certain conditions frustration as a consequence of withholding food reward can operate as a rather potent drive.

Although the data from the extinction phase of this experiment do seem to support the notion that the click serves as a secondary reinforcer, these results can be explained equally well with reference to the hypotheses mentioned in conjunction with Bugelski's (1938) extinction experiment. Such explanations would seem more parsimonious than would the utilization of the rather nebulous secondary reinforcement explanation.

SUMMARY AND CONCLUSIONS

An experiment was conducted to test the effect of a previously neutral stimulus upon the acquisition of a response. The acquisition data showed no measurable increase in response strength due to the additional stimulus. However, the presence of the previous neutral stimulus during extinction increased the number of extinction responses.

This experiment provides no evidence that the click, a previously neutral stimulus, after being paired with food, acts as a secondary reinforcer during acquisition. Although it does seem to act as a secondary reinforcer during extinction, the extinction results can be explained without reference to a secondary reinforcement concept. It would seem that the entire notion of secondary reinforcers should be carefully re-examined. Perhaps the role of previously neutral stimuli, such as clicks and the like, should be examined in terms of functional relations, without the surplus label, secondary reinforcer being attached to them.

REFERENCES

- ADELMAN, H. M., & MAATSCH, J. L. Learning and extinction based upon frustration, food reward, and exploratory tendency. *J. exp. Psychol.*, 1956, 52, 311-315.
- BITTERMAN, M. E., FEDDERSEN, W. E., and TYLER, D. W. Secondary reinforcement and the discrimination hypothesis. *Amer. J. Psychol.*, 1953, 66, 456-464.

- BUGELSKI, B. R. Extinction with and without sub-goal reinforcement. *J. comp. Psychol.*, 1938, 26, 121-124.
- BUGELSKI, B. R. *The psychology of learning*. New York: Holt, 1956.
- DEESE, J. *The psychology of learning*. New York: McGraw-Hill, 1952.
- ESTES, W. K. Stimulus-response theory of drive: a preliminary formulation. Paper presented at the Nebraska Symposium on Motivation, 1958.
- HULL, C. L. *A behavior system*. New Haven: Yale Univer. Press, 1958.
- MACKINTOSH, IRENE. The resistance to extinction of responses acquired under irregular conditions of learning. *J. comp. physiol. Psychol.*, 1955, 48, 363-370. (a)
- MACKINTOSH, IRENE. Acquisition latencies for responses acquired under conditions of total and partial reinforcement. *Trans. Kans. Acad. Sc.*, 1955, 58, 557-560. (b)
- MELCHING, W. H. The acquired reward value of an intermittently presented neutral stimulus. *J. comp. physiol. Psychol.*, 1954, 47, 370-373.
- MILLER, N. E. Studies of fear as an acquirable drive: I. Fear as motivation and fear-reduction as reinforcement in the learning of new responses. *J. exp. Psychol.*, 1948, 38, 89-101.
- MYERS, J. L. Secondary reinforcement: A review of recent experimentation. *Psych. Bull.*, 1958, 55, 284-303.
- SHEFFIELD, VIRGINIA F. Extinction as a function of partial reinforcement and distribution of practice. *J. exp. Psychol.*, 1949, 39, 511-525.
- WHITE, R. T. Analysis of the function of a secondary reinforcing stimulus in a serial learning situation. Unpublished doctoral dissertation, Univer. of Buffalo, 1953.

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HABIT REVERSAL AS A FUNCTION OF IRREGULAR DRIVE AND REWARD¹

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McClelland's proposition (1951), that stable learning in everyday life is a product of disorderly training conditions, has led to a number of laboratory studies (McClelland & McGown, 1953; Mackintosh, 1955; McNamara & Wike, 1958; Brown & Bass, 1958; Wike, et al, 1959). Since the first three experiments, all confirmatory, have been summarized elsewhere (McNamara & Wike, 1958), we shall confine our review to the latter studies. Brown and Bass (1958) trained hungry rats to traverse runways under variable (V) and constant (C) cue and response conditions. The Ss were then extinguished: half of Ss in each training group being run under C condition and half under V, i.e., the design was of the balanced factorial type. The most prominent findings were that Ss with V cue and response conditions *in extinction* had shorter starting and running times than Ss extinguished under C conditions, suggesting the importance of the experimental conditions at the time of extinction rather than during the training conditions as stressed by McClelland.

Wike, et al (1959), using thirst as a drive and a runway apparatus, conditioned Ss under V vs. C drive, V vs. C amount of water reward, and V vs. C response procedures. The Ss were deprived of water for 22 hr. and V drive was achieved by giving different watering periods prior to the runs; 0 or 5 or 10 min. in the V group vs. 5 min. in the C drive group. The Ss in the C reward group always had 2cc. of water, while the V Ss randomly received 0. 5 or 2 or 3.5 cc. on different trials. Variation in response was instituted by inserting hurdles in the runway on a random half of the trials. After training, extinction trials took place with C drive and C response conditions. In acquisition the V response Ss ran slower than those with C response, and while V drive led to consistently faster running, this latter result failed to attain statistical significance. In extinction Ss which had been trained with either V drive or V reward ran reliably faster than their controls.

The purpose of the present study was two-fold: first, to investigate the effects of V drive and V reward upon the acquisition and reversal of a spatial response in a single unit T-maze; and, second, by means of a balanced design, to determine the relative importance of training

¹ This investigation was supported by a research grant (M-1967) from the National Institute of Mental Health, Public Health Service.

and habit reversal conditions upon performance in habit reversal. In short, the study attempted to assess the generality of the findings of Brown and Bass and Wike, et al, which were reviewed above.

METHOD

Subjects

The Ss were 48 female experimentally naive Sprague-Dawley albino rats of 135-150 days of age. Twelve Ss were randomly assigned to each of the four training conditions described below.

Apparatus

A gray alley-type T-maze, made of 1 in. x 6 in. pine boards was used. The starting box was 8.5 in., the stem was 28 in., and the side arms were 36 in. long. The 15 in. x 9.5 in. goal boxes formed an "L" with the side arms so that S turned into the goal box in the same direction as its choice point turn. The S's progress through the maze was controlled by four Plexiglas drop doors. The maze rested upon 30 in. high tables and was illuminated by two 90-w. fluorescent tubes placed 4.5 ft. overhead. A copper drinking tube, which was attached by a rubber connection to a 10-ml. burette, projected into the far end of the goal box.

Procedure

On Day 1 4-5 Ss were placed together in living cages, given an abundant supply of Purina Fox Chow pellets, and deprived of water for 23 hr. Throughout the study food pellets were always available in the living cages. On Days 2-5 Ss were handled for 5 min. per cage and were watered for 60, 50, 40, and 30 min., respectively. On Day 6 Ss were put into the compartments of a black handling cage located in the living area, and were permitted to drink from individual water bottles for 5 min. Five min. later they were taken to the maze, which was set up in a different room, where they explored in groups of 4-5 for 5 min. Fifteen min. afterwards they were watered for 25 min. in their home cages. This watering procedure, a 5-min. preload before running and 25-min. watering after the daily trials, was the C drive condition. Days 7-8 involved the same procedure except that Ss explored in groups of 2-3. On all exploratory runs the water tubes were filled and accessible in both goal boxes. On Days 9-13 Ss had a single preference trial given under the C drive conditions, and they were rewarded with 2cc. of water, the C reward condition, regardless of the direction of turn.

Training began on the next day. For 16 days Ss had one maze trial daily, using the correction method of running. All Ss were rewarded on their nonpreferred side. Following a 2x2 factorial design, four motivational treatments were in effect, i.e., the four combinations of C and V drive and reward. The C drive Ss were prewatered for 2.5

min. and drank 15 min. after their trials for 27.5 min.; the V drive Ss were prewatered for 0 or 2.5 or 5 min., and 15 min. later drank for 30 or 27.5 or 25 min. The C reward Ss always had a 1.5 cc. reward; the V reward Ss received either 0.5 cc or 1.5 cc. or 2.5 cc. drink.² The quasi-random patterns of preloads and rewards in the V drive and reward groups, which have been described elsewhere (Wike, et al, 1959), were so arranged that Ss in all groups drank for a total of 30 min. and received a 1.5 cc. reward on the average daily.

Since the correction method failed to produce learning (Ss actually running more frequently to the nonpreferred, incorrect side at the termination of training than they did at the outset), Ss were given 25 days of training in which one free and one forced trial were administered daily. During this period S was not permitted to correct its choices, and when S made a wrong turn it was confined in the empty incorrect goal box for 20 sec. After the free trial, S was kept in the handling cage for 20 sec. before being giving the forced trial. The differential motivational conditions were continued during this acquisition training with the noncorrection technique.

Habit reversal occurred in the following 10 days. Reward was shifted to the opposite goal box for each S and two trials, one free and one forced, were run daily. Three Ss from each of the four training conditions were randomly assigned to each of the four motivational conditions in habit reversal, yielding a 2⁴ factorial plan with drive in training, reward in training, drive in reversal, and reward in reversal as the main effects.

RESULTS

The mean errors per blocks of five trials during the noncorrection training as related to the motivational conditions in training are shown in Figure 1. The only discernible trend in this period was that the V drive Ss consistently committed fewer errors. An analysis of variance, however, disclosed that none of the main effects nor their interactions were statistically significant. The reversal conditions were also included in this analysis, and their lack of significance lends credence to the view that the reversal groups were comparable in their training performance.

The mean perseverative errors in habit reversal, where perseverative errors were defined as the number of incorrect free responses in reversal prior to the occurrence of the first correct free turn, were subjected to an analysis of variance. This analysis revealed that Ss with V drive in reversal made significantly more such errors than C drive Ss: $F=6.28$; $df=1,32$; $P<.05$. A second finding was that the interaction

² The preloads and reward magnitudes were reduced at the beginning of training because the smaller female Ss used in this study drank less readily during the preference runs than the males in the first experiment.

between drive in training, drive in reversal, reward in training, and reward in reversal was significant: $F=6.35$; $df=1,32$; $P<.05$. Inspection

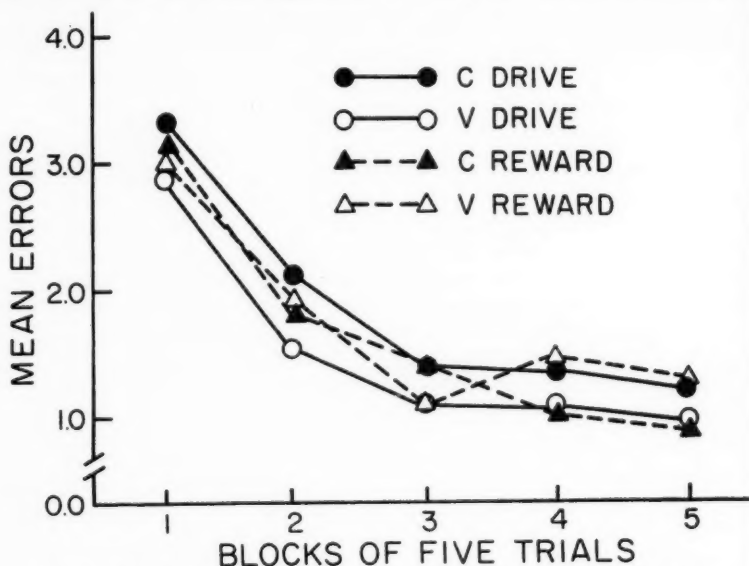


Fig. 1. Mean errors per five-trial block in training as a function of training conditions.

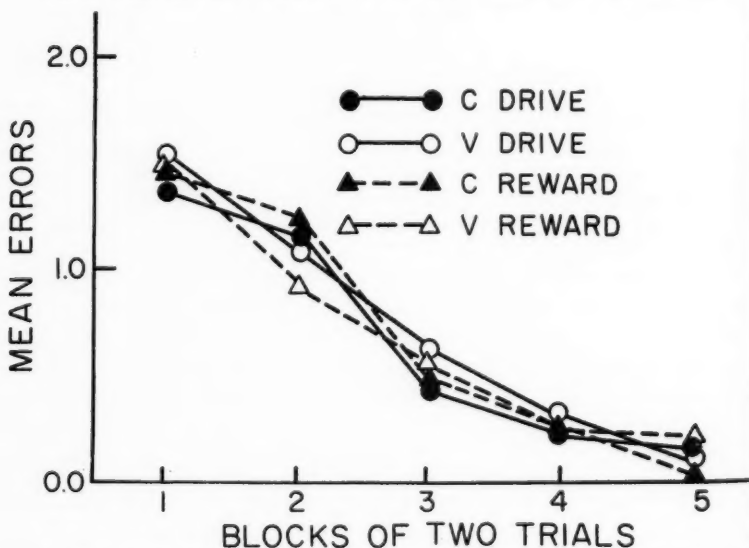


Fig. 2. Mean errors per two-trial block in habit reversal as a function of training conditions.

ing, and
Inspec-

tion of the interaction tables suggested no obvious interpretation for this complex interaction.

The average errors per two-trial blocks during habit reversal as related to the motivational conditions in training and habit reversal are presented in Figures 2 and 3. An analysis of variance of these observations disclosed only one significant effect: Ss which had V

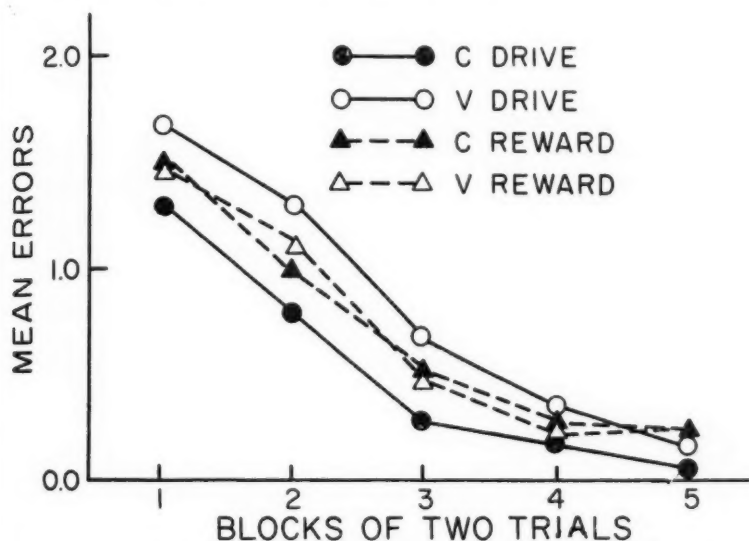


Fig. 3. Mean errors per two-trial block in habit reversal as a function of reversal conditions.

drive in reversal erred more frequently than those run under C drive ($F=8.50$; $df=1,32$; $P<.01$). As is obvious from Figure 2 there were no consistent carry over effects from the different motivational conditions imposed in training upon performance during the habit reversal phase.

DISCUSSION

The main findings of the present experiment were that: 1. V drive and V reward did not produce significant differences in the acquisition of a position response in a T-maze from C drive and C reward, although V drive Ss consistently committed fewer errors; 2. in habit reversal V drive during reversal led to significantly more perservative errors; 3. the drive and reward conditions in training had no effect upon reversal performance; and 4. V drive in reversal resulted in more errors in reversal. Finding 1 is identical with previous results with a runway apparatus (Wike, et al, 1959). Comparison of the habit reversal

results with the runway extinction findings is difficult, however, since the two studies differed not only along a dimension of task difficulty, but also with respect to the use of a balanced design in the present study at the time of reversal. At the very least, the effectiveness of the V drive condition in reversal upon reversal performance (findings 2 and 4) and the lack of influence of training motivational conditions upon reversal performance (finding 3) would call into question McClelland's assertion (1951) that V training conditions, per se, produce highly stable learning—an assertion which we accepted earlier (Wike, et al, 1959). In addition, the last three findings are in full accord with the results of Brown and Bass (1958) who found that V cue and response conditions during extinction prolonged extinction.

A theory of selective learning and habit reversal, which has been offered by Kendler and Lachman, would appear to encompass the present habit reversal results. They have proposed:

The S, in order to solve a discrimination problem, must acquire a stronger habit to approach the positive stimulus than to approach the negative stimulus. If after solving the discrimination, S is required to execute a reversal shift (i.e., learn to reverse his preference), then the approach response will have to be weakened below the strength of the approach response to the original negative stimulus. Any factor that would retard this weakening process should also retard a habit reversal (Kendler & Lachman, 1958, p. 584).

This interpretation assumes that habit reversal is a two-stage process: a first stage in which the originally correct habit is extinguished, and a second stage in which the formerly incorrect response must be strengthened. Kendler and Lachman believe that high drive at the time of reversal would affect performance in stage one by retarding reversal, presumably the excitatory potential for the originally correct response in a high drive group would be greater than that for a low drive group as a result of multiplying the equal habit strengths for the two groups by a larger drive factor for the high drive group. When stage one is complete, however, and the new correct response is dominant, then high drive should facilitate reversal.

Three findings in the present study fit this interpretation nicely: 1. Ss with V drive in reversal committed more perseverative errors during reversal; 2. these Ss made more errors in reversal; and 3. the reversal curves for the V and C reversal drive groups showed a definite trend toward convergence as reversal training progressed (cf. Figure 3). This interpretation of the present results entails an implicit assumption that the absolute drive level of the V drive Ss is greater than that for the C drive Ss. This assumption would not appear to be completely un-

warranted since the V drive Ss ran consistently faster in acquisition in the earlier runway study. As the number of trials and reinforcements was held constant in that study, it would seem reasonable to attribute this consistent but nonsignificant tendency for the V drive Ss to run faster to a higher drive level. Other indirect evidence in support of the assumption comes from Mandler's experiments (1957) in which it was demonstrated that irregular maintenance schedules increased the drive effect of the deprivation interval which was employed at the time of testing. It should be noted that Mandler's irregularly deprived Ss also showed retarded habit reversal performance in a four-unit successive T-maze when compared to Ss on a regular maintenance regimen.

Finally, consideration needs to be given to the fact that V reward had no differential effects in the present investigation. An obvious, possible explanation for this would be that the variations in reward magnitude, which were reduced from those used in the original runway experiment, were not large enough to produce differential effects upon behavior.

SUMMARY

A total of 48 thirsty rats was trained to make a spatial response in a single unit T-maze under conditions of variable (V) and constant (C) drive and reward conditions. V drive was achieved by giving different preloads of water prior to running, and V reward consisted of administering different magnitudes of water in the goal box. The C drive and reward groups received a C preload and C reward magnitude. After acquisition, habit reversal training took place, and a completely balanced factorial design with respect to the motivational conditions was enforced.

The main results were that: 1. V drive and V reward did not significantly affect the acquisition of the spatial response, although the V drive group consistently made fewer errors; 2. V drive in reversal led to a greater frequency of perseverative errors; 3. V drive and V reward in training had no effects upon reversal performance; and 4. V drive in reversal resulted in more errors in the habit reversal period. These results were compared with previous studies involving irregular training conditions and discussed in terms of Kendler and Lachman's theory of selective learning and habit reversal.

REFERENCES

- BROWN, J. S., & BASS, BETTINA. The acquisition and extinction of an instrumental response under constant and variable stimulus conditions. *J. comp. physiol. Psychol.*, 1958, 51, 499-504.
- KENDLER, H. H., & LACHMAN, R. Habit reversal as a function of schedule of reinforcement and drive strength. *J. exp. Psychol.*, 1958, 55, 584-591.
- MACKINTOSH, IRENE. The resistance to extinction of responses acquired under irregular conditions of learning. *J. comp. physiol. Psychol.*, 1955, 48, 363-370.

- MANDLER, JEAN M. Irregular maintenance schedules and drive. *Science*, 1957, 126, 505-506.
- McCLELLAND, D. C. *Personality*. New York: Dryden, 1951.
- McCLELLAND, D. C., & McGOWN, D. R. The effect of variable food reinforcement on the strength of a secondary reward. *J. comp. physiol. Psychol.*, 1953, 46, 80-86.
- McNAMARA, H. J., & WIKE, E. L. The effects of irregular learning conditions upon the rate and permanence of learning. *J. comp. physiol. Psychol.*, 1958, 51, 363-366.
- WIKE, E. L., KINTSCH, W., & GUTEKUNST, R. The effects of variable drive, reward, and response upon instrumental performance. *J. comp. physiol. Psychol.*, 1959, 52, 403-407.

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CURRENT AND PROJECTED STATUS OF SEMANTIC DIFFERENTIAL RESEARCH^{1, 2}

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This paper will provide a progress report of research trends involving the semantic differential since the publication of *The Measurement of Meaning* (Osgood, 1957). Osgood and other principal investigators have, of course, had a continuing interest in the attempt to identify what they have referred to as the basic dimensions of meaning in language behavior. In the attempt to purify the factor structure and to isolate more sensitive scales to represent each factor, investigations have continued in which adjectives, concepts, subject population, and methods of factor analysis have been varied.

Especial interest has continued in the study of cross-cultural meaning systems (Kumata, 1957; Lambert, Hanelka, & Crosby, 1958; Triandis & Osgood, 1958). Analysis of the data from a half dozen diverse culture and language groups has indicated a surprising degree of similarity in the basic semantic dimensions within which meaningful judgments are made. There are occasional exceptions, such as Suci's (1958) investigation in which he found that similar semantic spaces prevailed among Zuni, Hopi, and Spanish subjects, but the Navahos deviated noticeably, suggesting that a large amount of attribute space needs to be defined with further research.

Cross cultural generality of meaning systems is of considerable theoretical interest, of course. Of more practical importance is the possibility of devising common measuring instruments for comparing the attitudes, values and beliefs of peoples throughout the world. It would be valuable to put these cross-cultural studies on a systematic basis. One can envision the theoretical and practical usefulness of a "world semantic atlas," with semantic factors determined for each language/culture group using the same standard sampling procedures and with semantic differential instruments derived from these factors. If the major factors proved to be essentially the same, regardless of race, place, or nationality, then the measuring instruments derived from the factors would be

¹ Presented at Division 12 Research Symposium, APA meeting, Cincinnati, September, 1959. This paper is part of a trilogy: Dr. Edward Ware, University of Illinois, introduced the semantic differential in the context of psycholinguistic research, while Dr. John Carroll, Harvard University, concluded with a critique of the method.

² I would like to thank Dr. Charles E. Osgood for his invaluable assistance in acquainting me with recent and planned research.

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equivalent, despite differences in specific scales, and comparable measures could be obtained.

The following are merely examples of the many exciting problems that could be investigated when equivalent semantic differentials are finally available. (1) *National and ethnic stereotypes*: It would be valuable to know in comparable quantitative terms, how people in various countries perceive each other, and how these images change under the various pressure of events (for instance, the Chinese Communist invasion of Tibet). (2) *Various aspects of the American Way of Life*: It should be possible to determine which aspects of our way of life are understood and which misunderstood, which appreciated and which unappreciated, and on what grounds (e.g., dollars, CARE, democracy, coca cola, New York City).

Apart from the fact that such investigations would make explicit subtle attitudes, meanings, values, and beliefs held by people in other countries, the simple demonstration of a shared semantic framework for human thinking, regardless of race or nationality, should in itself encourage international understanding, and the clear isolation of differences in concept and scale meanings should enable us to do a better job of talking to each other.

During the past two years Osgood and Ware have completed a joint study on the measurement of human values with Professor Charles Morris (1958). College students judged descriptions of Morris' 13 "ways to live," (cf. his book on *Varieties of Human Value*) against a long form of the semantic differential. Factor analysis of the interscale correlations indicated three independent dimensions along which human values distribute themselves: a "successfulness" factor (which included the first three factors usually obtained, evaluation, potency, and activity); a "sociability" factor; and a "stability" factor. Several questions arise from these findings; for instance, are there cultural differences in value factors for Morris' "ways to live," and what are the individual differences among Americans relative to socio-economic or educational groupings? An implication of this study which will be discussed later is that subsets of concepts can appreciably alter the factor structure, indicating a need to develop semantic differentials for different classes of concepts.

In a recent cross-cultural visual-verbal synesthesia experiment, Osgood (unpublished) had Navajo and Mexican-Spanish subjects judge verbal concepts against visual-binary (pictorial opposite) scales. Thus, instead of the bipolar words *thin-thick*, the subject sees a *thin* line paired with a *thick* line, and he simply points to whichever drawing seems to 'go best' with the concept being judged. The difference between the drawings in each pair is restricted to a single dimension, e.g., angularity, size, nearness, etc. The study had several purposes: to check the generality of semantic factors; to study the cross-cultural generality of visual synesthesia; to see if those terms in foreign cultures selected as translation-equivalent to verbal opposites in English, actually function as opposites

in the meaningful judgments of non-Anglo subjects. Results suggest a definite trend toward consistent intra-cultural synesthesia and toward only slightly lesser inter-cultural agreement. These results strengthen the possibility that a visual-metaphorical ("culture-free") differential may eventually be developed.

People working with the semantic differential have been increasingly interested in the personality and psychotherapy area. Although there are hundreds of "personality tests" available, there does not seem to be any standardized way of simply describing (and hence accurately communicating) the individual personality. "Personality" can be regarded as essentially a meaningful construct developed out of interpersonal interactions, and therefore the general techniques of semantic measurement should be applicable. And it is usually the connotative meaning ("feeling tone") rather than the denotative meaning (name, physical, characteristics, etc.), of the person in which there is interest. Factor analysis of judgments of the concept ME by 100 subjects, reported in *The Measurement of Meaning*, yielded six suggestive independent factors. More recently a new differential based on the framework provided by these provisional factors was constructed. Three clearly defined and stable factors were found: "Morality" (analogue of the general evaluative factor); "volatility" (analogue of the general activity factor); and "toughness" (analogue of the usual potency factor). Three additional factors were suggested.

It is now proposed to clarify this "personality factor structure," using new scales, subjects, concepts and factor analysis. It is then planned to test the stability of this system in a variety of situations where personality descriptions are typically made, e.g., in describing personalities as revealed in job or clinical interviews. It is hoped there can eventually be developed a short form differential (no more than 18 scales) that can serve as a standardized instrument for describing the "personality" of self or others in a wide range of situations.

Using this instrument, it is planned to continue the search for the bases of individual differences in semantic factor structure. Heretofore, studies have been made to determine the generality of the factor structure. In all these comparisons it was the *samenesses* across groups rather than *differences* that have been impressive. Recently single-factor analyses have been undertaken. For instance, intelligence may affect the factor structure. A study by Ware (1958) found no appreciable influence of intelligence on the factor structure; however, it is possible that differences in intelligence may appear when concepts of sufficient complexity are used. It is also possible that differences in intelligence are reflected in fineness of discrimination (i.e., fineness of the 'grain' of the semantic space) rather than in the nature and number of semantic factors per se. Differences related to age may be studied (does the semantic space become increasingly differentiated with increasing maturity and then con-

tract with old age?), as well as differences related to various measurable personality traits (do high anxiety people show constriction in semantic judgements, as reflected by voluntary restriction to the 3-4-5 categories on the differential?).

A problem that falls in this area of individual differences is the relation between George A. Kelly's work and the semantic differential. Kelly has developed what he calls a "Rep Test," in which, essentially each individual subject develops his own judgmental dimensions by making triadic comparisons among significant persons in his life, e.g. for the set "My Mother," "My Wife," and "My Boss," the first two may be judged more similar "because they are both more emotional;" so *emotional-unemotional* becomes one dimension in this subject's judgmental space. The dimensions derived from individual subjects by this procedure may actually be variations on the general factorial themes isolated in Osgood's work. If this can be shown to be the case, then use of the personality semantic differential has at least two advantages: it provides for comparability across subjects and it reduces the labor of analysis manifold, because separate factoring is no longer required for each individual person studied.

One of the more exciting studies under consideration relates to the development of a personality description instrument that would combine the subtlety of projective techniques with the objectivity and ease of application of present forms of the semantic differential. With the present scales it must be assumed that the subject is being honest—but it is easy for him to malingering. The proposal is to devise scales based on visual and verbal metaphors which can be shown to represent the same personality factors as the ones now being used. For example, in the factor analysis of interpersonal concepts mentioned above, the attempt was made to identify scales which would get at the same factors through verbal metaphors. Volatility was best represented by scales such as *calm-excitable* and *emotional-unemotional*, but also by the subtler scale *fluctuating-level*; morality was best represented by the scales *moral-immoral* and *reputable-disreputable*, but also by the metaphor *straight-twisted*.

Drs. Alexander and Husek (1959) have been working on the development of a semantic measure of situational anxiety. They have tentatively developed three anxiety tests which are able to differentiate between "anxious" and "nonanxious" states of the same individuals, as well as between anxious and nonanxious groups. They are also working on a subtle form which they hope will detect malingers. For instance, in place of obvious adjectives such as "jittery" or "frightened" other adjectives such as "curved" or "wet" have been demonstrated to provide an index to anxiety in some concept-scale combinations. Much additional research remains to be done before the Anxiety Differential can be used as a diagnostic instrument.

Another approach to the problem of subject honesty was suggested by the semantic analysis of "The Three Faces of Eve." Osgood and Luria predicted, on the basis of the differentials given to the three aspects of this personality, that the emergent personality, Jane, was not a genuine personality. This prediction was later verified when it was found that Jane was simply Eve Black playing the role of a person acceptable to the therapist. The original clue was that the semantic structure derived from Jane's ratings were collapsed or oversimplified, that is there was a reduction in discrimination so that it became almost entirely evaluative in nature. Evidence indicates that when people role play there is a detectable simplification of the semantic structure. This would seem to have interesting implications for studies such as measuring change associated with psychotherapy. Can the differential distinguish between such superficial changes and those more deepseated?

The author has been preoccupied several years now with the use of the differential to evaluate the nature and function of dreams and more specifically, tests of the psychoanalytic theory of a dream censorship mechanism (Moss, 1957; 1959a; 1959b). Briefly, use was made of hypnotized subjects who had the seeming ability to "dream" or to interpret dreams while hypnotized. The differential was used to measure the semantic distance between the latent and manifest content under conditions of anxiety and nonanxiety. Psychoanalytic theory would predict that there should be a greater semantic distance (that is, disguise) between symbol and covert content under conditions of anxiety. Thus far the results have *not* substantiated this prediction; however, this may reflect certain limitations of the semantic differential. The differential measures aspects of connotative meaning and there seems to be fairly substantial evidence that the emotion associated with a dream is subject to less distortion than other aspects. What apparently is needed to evaluate the assumed disguise function of dream symbolism is a "denotative differential," one that will measure the physical attributes of meaning. Another methodological limitation is that the relationship between symbol and latent content is typically tenuous, and, therefore, a denotative differential would have to be extremely sensitive to relatively minute and individualized aspects of meaning rather than measuring the common variance among groups of subjects. This type of problem points up limitations of present forms of the differential.

A picture of the diversity of research recently completed or underway might be captured with brief sketches of a number of additional representative studies. Keswich (1958), put the semantic differential to commercial use. A survey was conducted of the public's image of utilities such as the electric light, gas, and telephone companies, their servicemen, and appliances (gas versus electric cooking ranges). A basic question concerns the intelligent selection of concepts most relevant or sensitive to changes expected with psychotherapy. Luria (1959) indicates in a recent publication that prospective therapy subjects can be

discriminated from normals on the basis of their relative *devaluation* of self and parent concepts. Somewhat surprisingly, therapy resulted in an increase in positive attitude toward 'self' but not toward the parents. Asher and Evans (1958) used the differential to show significant shifts of audience attitude toward emotionalized concepts such as "Negro" and "the desirability of public discussion of sex problems" as the result of educational television programs. Ervin (1959) has been interested in the development of meaning in children. Children were shown paired objects differing in only one attribute (e.g., physical objects differing in weight or size or strength). The younger children showed a marked tendency to confuse denotative bipolar terms. For instance, when these objects differed in size, the heavier was also judged bigger and stronger. Results suggest that in young children many dimensions of meaning are virtually synonymous—and the synonymities seem to correspond to Osgood's general factors. This raises the question of how these are differentiated as experience increases.

Cook (1959) explored the relationship of the meaning of selected concepts such as "myself as a student" and "the ideal student" to the achievement and scholastic ability of college students. He found that meaning is highly related to achievement. As much as 15 to 20 per cent improvement was gained in the prediction of achievement with the ACE total score when certain meaning variables were added. Katz (1959) studied "Meaning as a correlate of marital success," and found consistent differences in the semantic structure of troubled as contrasted to untroubled married partners. Troubled partners are more discrepant than untroubled couples on *all* concepts judged and over all factors, although the *greatest* discrepancy was on concepts relevant to the marriage relationship. Prothro and Keehn (1957) conducted a study of the national stereotypes of Arab students (e.g.: how they viewed Turks, Germans, Italians). A factor analysis of the ratings yielded three factors corresponding to Osgood's basic dimensions. The ratings allowed description of the national stereotypes of the subjects in rather clear detail.

Nunnally and Kittross (1958) employed the semantic differential in the assessment of public attitudes towards mental health professions. They found a generally positive attitude towards all professions and particularly those identified with physical medicine. Nebergall (1958) investigated clarity of communication, the semantic differential being employed to measure the intended and conveyed meaning of messages. The semantic differential distinguished between communicators in their effectiveness in conveying intended meanings. Gwaltney (1959) differentiated between chronic and acute mental patients on the basis of their ratings of concepts reflecting the strength of their relative identification with hospital and community. The study suggests the possibility of selecting patients possessed of attitudes receptive to rehabilitation services.

CONCLUSION

It should be reemphasized that this has been intended as a brief progress report. Quite apparently psychologists have been quick to adopt the semantic differential as a tool of broad usefulness. In some ways the many applied studies may be viewed as premature, preceding the full development of the semantic differential as a measuring instrument. Many methodological problems remain. Messick (1957), for example, investigated the assumption of equal unit intervals within and between scales, through use of the method of successive intervals. Between scale intervals were found to be quite similar; a small distortion within scales was not sufficient to necessitate discarding the equal interval assumption.

The validity of the dimensions of meaning originally identified by Osgood and his associates has been subjected to test by Staats and Staats (1957). They paired adjectives representative of one or the other factors with a stimulus and through a classical conditioning procedure, the stimulus acquired the meaning reflected by this dimension. Each word was paired only once with the stimulus in order to avoid conditioning any specific word to the stimulus. Successful conditioning occurred with each of the three major dimensions.

The question of how well the semantic space has been sampled remains unanswered. It is quite possible that with an increased number of adjectival scales and appropriate selection of concepts, a far larger number of independent dimensions along which meaning judgments vary would be identified.

The knotty problem of the "meaning of meaning" itself has not been resolved to everyone's (anyone's) satisfaction (Carroll, 1959). However, the results of these studies do testify to the fact that the semantic differential is measuring "something" consistently and in meaningful fashion, and that, in this respect, it is already a useful instrument.

REFERENCES

- ALEXANDER, S., & HUSEK, T., The development of a measure of situational anxiety. Institute of Communications Research, Univ. Ill., 1959, (unpublished).
- ASHER, J. J., & EVANS, R. I., An investigation of some aspects of the social psychological impact of an educational television program, Educational Television and Radio Center, Ann Arbor, Mich., 1958, (unpublished).
- CARROLL, J. B., Some cautionary notes on the semantic differential, APA Symposium, Cincinnati, 1959.
- COOK, D. R., A study of the relationship of the meaning of selected concepts to achievement and ability, Unpubl. Ed.D. thesis, U. Indiana, 1959.
- ERVIN, S., Study pertaining to semantic dimensions used by young children, Unpubl. Ph.D. thesis, U. Calif., Berkeley, 1959.

- GWALTNEY, H. O., Reference identification as a variable in convalescence and chronicity of mental hospital patients, Unpubl. Ed.D. thesis, U. Missouri, 1959.
- KATZ, M., Meaning as a correlate of marital success, Unpubl. Ph. D. thesis, Teachers College, Columbia U., 1959.
- KESWICH, G. (Needham, Louis, & Brorby, Inc., Prudential Plaza, Chicago 1, Ill.), Public utilities opinion survey, 1958, (unpublished).
- KUMATA, H., A factor analytic investigation of the generality of semantic structure across two selected cultures, Unpubl. doctoral thesis, U. Ill., 1957.
- LAMBERT, W. E., HANELKA, J., & CROSBY, C., The influence of language-acquisition contexts on bilingualism, *J. Abnorm. Soc. Psychol.*, 1958, 56, 239-244.
- LURIA, Z., A semantic analysis of a normal and a neurotic therapy group, *J. Abnorm. Soc. Psychol.*, 1959, 58, 216-220.
- MESSICK, S. J., Metric properties of the semantic differential, *Educ. Psychol. Measmt.*, 1957, 17, 200-206.
- MORRIS, C., OSGOOD, C. E., & WARE, E. F., Analysis of the connotative meanings of a variety of human values as expressed by American college students, 1959.
- MOSS, C. S., Dream symbols as disguises, *Etc: J. Gen. Sem.* 1957, summer, 14, 267-273.
- MOSS, C. S., Dream symbols as disguises II, *Etc: J. Gen. Sem.*, 1959, in press. (a)
- MOSS, C. S., Mediation theory of symbolism, 1959, (unpublished). (b)
- NEBERGALL, R. E., An experimental investigation of rhetorical clarity, *Speech Monographs*, XXV, 1958, 243-254.
- NUNNALLY, J., & KITTROSS, J. M., Public attitudes toward mental health professions. *Am. Psychologist*, 1958, 13, 589-594.
- OSGOOD, C. E., SUCI, G., & TATNENBAUM, P., *The Measurement of Meaning*, Univ. Ill. Press, Urbana, 1957.
- OSGOOD, C. E., The cross-cultural generality of visual-verbal synesthetic tendencies (unpublished).
- PROTHRO, E. T., & KEEHN, J. D., Stereotypes and semantic space, *J. Soc. Psychol.*, 1957, 45, 197-209.
- STAATS, C. K., & STAATS, A. W., Meaning established by classical conditioning *J. Exp. Psychol.*, 1957, 54, 74-80.
- SUCI, G. J., A comparison of semantic structures in American southwest culture groups, project sponsored by Committee on Linguistics and Psychology of SSRC, 1958, (unpublished).
- TRIANDIS, H. C., & OSGOOD, C. E., A comparative factorial analysis of semantic structure in monolingual Greek and American college students. *J. Abnorm. Soc. Psychol.*, 1958, 57, 187-196.
- WARE, E. E., Relationship of intelligence and sex to diversity of individual semantic meaning spaces, Unpubl. Ph.D. thesis, Univ. Ill., 1958.

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A NEW SCHEME FOR THE INHERITANCE OF INTELLIGENCE^{1,2}

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Even a superficial survey of the literature dealing with the inheritance of "intelligence" and "feeble-mindedness" reveals a certain vagueness and confusion. A great variety of genetic systems has been postulated by various writers. Jennings (1930, pp. 12-25), for one, assumes feeble-mindedness to be a homozygous recessive condition. To explain how two defective parents may have a superior offspring, he further assumes that the defective genes are located in different pairs. When a defective offspring results from defective parents, it is assumed that the parental genes were in the same position. In addition, he assumes unusual combinations of supplementary genes to explain the superiority of genius of offspring of mediocre or inferior parents.

Martin and Bell (1943, pp. 154-157) have published a pedigree which they believe shows that the mental defect is due to a sex-linked recessive gene with the minor exception of slight mental deficiency in two females in which case the causal gene was said to be incompletely recessive.

Limiting himself to the heredity of phenylpyruvic oligophrenia, Jervis (1939) suggests the operation of an autosomal recessive gene in the manifestation of this particular type of defectiveness.

Penrose (1941, pp. 359-364) believes that severe cases of mental deficiency may be due to recessive factors or fresh gene mutations. However, some mild types may be the result of incomplete dominance and variable expressivity of genetic factors (Penrose 1938, p. 291).

According to Gates (1946, p. 1150),

While mental and physical abnormalities are generally

¹ With so many competing genic explanations to account for the inheritance of intelligence, the writers decided to try their hand at the game. We achieved our results in the manner of our predecessors; no more than they, did we study successive generations of humans by relating their test performances to relevant variables. This is what we did not do. What we did do was to simply sit down and construct a purely hypothetical scheme, which works so well that it can encompass a wide range of facts. Furthermore, it is, in a theoretical sense, more economical than other theories. The worst thing to be said against it is that it is free theoretical construction, a kind of science fiction.

² This study was supported by a grant from the research fund of Dr. Margaret Habein, Dean of Liberal Arts, to whom grateful acknowledgement is hereby expressed.

sibility of the derivation of an idiot, IQ 0-20; imbecile, IQ 20-60; moron, IQ 60-70; borderline, IQ 70-80; dull normal, IQ 80-90; normal, IQ 90-110; superior, IQ 110-120; very superior, IQ 120-140; up to near genius or genius, IQ above 140. There is no need to make additional assumptions proposed by various authors (e.g. differential positions of gene pairs, variability in "expressivity," etc.). In other words, the explanation here proposed is scientifically more conservative. It is simplicity itself and yet it covers the full range of facts, an achievement which the variety of theories considered above makes at the expense of discrepant or contradictory handling of *segments* of that range of facts.

Within the general framework of traditional genetic theory, the genic explanation for the inheritance of intelligence here presented is believed to be deserving of consideration alongside those with which it has been compared. At least, it is no less hypothetical than the others, and, after all, it does handle the data, no matter what those data might be. We offer this schema then, as the last word in genetic explanations of the relationships between the IQ's of parents and their offspring.

SUMMARY

A variety of theories pertaining to the inheritance of intelligence is examined and critized and a new scheme is presented. This assumes four pairs of genes acting cumulatively and accounts for the possibility of offspring ranging from the idiot to the genius level when the parents are dull normal. The schema is offered as the last word in genic explanations of the relationships between the IQ of parents and their offspring.

REFERENCES

- GATES, R. R. *Human genetics*. New York: Macmillan, 1946. (Vols. I & II) Pp. xvi and 1518.
- JENNINGS, H. S. *The biological basis of human nature*. New York: Norton, 1930. Pp. xvi and 384.
- JERVIS, G. A. A contribution to the study of the influence of heredity on mental deficiency. The genetics of phenylpyruvic oligophrenia. *Proc. Amer. Ass. ment. Def.*, 1939, 44, No. 2, 13-24.
- MARTIN, J. P., and BELL, J. A pedigree of mental defect showing sex-linkage. *J. Neurol. Psychiat.* 1943, 6, 154-157.
- PENROSE, L. S. Genetical problems and mental deficiency. *Eugen. Rev.*, 1938, 30, 291.
- PENROSE, L. S. Inheritance of mental defect. *Sci. Mon.*, 1941, 52, 359-364.

The Psychological Record, 1960, 10, 58.

Galanter, Eugene (Ed.) *Automatic Teaching: The State of the Art*. New York, John Wiley and Sons, Inc., 1959. Pp VIII+ 198. \$3.25.

Automatic Teaching is a collection of sixteen papers offered at a Symposium at the University of Pennsylvania under the auspices of the Air Force Office of Scientific Research.

The volume begins with a paper by Galanter entitled, "The Ideal Teacher." The purpose of this chapter seems to be to establish the set for the remaining papers. However, most of the succeeding papers fail to follow up any of the ideas expressed in this chapter.

The second chapter by Gagne and Bolles entitled, "A Review of Factors in Learning Efficiency," is a well prepared summation of our present knowledge of transfer of training. It could, however, have been written without reference to the topic of the Symposium. There is little if any relationship between this paper and "teaching machines."

Chapters three through thirteen offer reports on attempted programs using various automatic teaching devices. Illustration of programs in such diverse fields as Air Force pilot training, grade school arithmetic and spelling, college courses in psychology, and electronic maintenance are found in these chapters. Each author gives an appraisal of his work, and each tends to support the editor's statement found in the introduction: "... papers ... generate more problems than they illuminate."

The last three papers by Zeaman, Kendler, and Pressey are not reports of individual research, but rather papers designed to raise purely theoretical questions. Zeaman is concerned with the relationship, in dealing with teaching machines, of three basic experimental paradigms: free operant, controlled operant, and classical conditioning. Kendler calls attention to the need for better theories of behavior, particularly with respect to transfer and symbolic processes, in order to make the best use of teaching machines. Pressey, in a summation of the Symposium, offers the suggestion that very capable work has been done in the fields of education, educational psychology, and industrial psychology and that this work has been ignored in the recent upsurge of interest in teaching machines. He deplores this situation and chastises the Symposium for ignoring important data.

Most books, the chapters of which are written by different authors, suffer from a lack of continuity. *Automatic Teaching* suffers more than most. The papers by Galanter, Gagne and Bolles, Skinner, Homme and Glaser, Zeaman, Kendler and Pressey are conspicuous by the fact that they are lucid.

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The Psychological Record, 1960, 10, 59-61.

Hook, Sidney (Ed.), *Psychoanalysis scientific method and philosophy*. New York: New York University Press, 1959. 370 pp.

Masserman, Jules H. (Ed.), *Individual and familial dynamics*. Science and psychoanalysis, vol. II. New York: Grune and Stratton, 1959. 218 pp.

The first book, which represents most of the proceedings of the second annual NEW YORK UNIVERSITY INSTITUTE OF PHILOSOPHY (1958), is a delightful smörgåsbord of intellectual opinions. All the contributors are able writers and, though some of the papers are mere vignettes, the collected effect is a sound one.

On the conservative and more sensible panel we find a host of writers. Among the Psychoanalysts, Hartman writes a sober and modern version of analytic doctrine, while Kubie is at his stimulating best. Of the Philosophers the most sophisticated seemed to me to be Hoppers, who was knowledgeable without being emotional; the most gentlemanly was of course Demos of Harvard, whose paper is a 'must.' Although he seemed to run on and on, Scriven showed a remarkable store of knowledge of the practical problems involved in a psychoanalysis, something which most of the philosophers lacked.

On the radical, or herring side of the table we have the extreme views of Nagel in philosophy, in a rather arbitrary denial of all of analysis. On the other side there was Kardiner in opinions so arbitrary and so firmly stated as to make most of the mixed participants blush. One defector to the enemy camp was Lazerowitz, a philosopher whose conversion would make most psychoanalysts blush. He says, page 148, "The metaphysician's theory is a subtle and remarkable work of verbal art. Its underlying mechanics are a grammatically changed language which is not meant for practical discourse but, like a work of art, is meant only for contemplation. It creates the appearance of a profound theory about the cosmos, while it actually expressed inner psychic dramas." Much of the remaining time of the book is spent in explaining him to the 'nice people' around, much as a doctor does with a publically grateful but still obviously much disturbed patient. His discourse is interesting but did not seem to appeal to either group, nor to me.

Hook, in a burst of kindness so necessary for the editor of this sort of book, says, page 221, "I find that I can as a rule understand what a philosopher says better than I can understand what a psychoanalyst tells me he is really saying. When I am in doubt as to what he is really saying, logical analysis is immensely more helpful than psychoanalysis." I am reminded of a professor of mine, in psychoanalysis, who

excused some of us during diagraming time with the statement, 'Diagrams are helpful to those who find diagrams helpful'.

Diagrams or not, logical analysis or not, psychoanalysis or not . . . it all seems to depend upon some point of view. Each paper as I read it seemed logical and even considerably true when directly engaged. When the book went down, or another paper was read, the same thing happened again. All were interesting and exciting but I could not help but recall the apparent seriousness of the authors, at least some of them. Danto, of Columbia was the only one to point out that the symposium sounded more like a shouting debate between 'Believers' and 'Non-believers' and I had that impression long before coming to his paper. There was really very little scientific discussion, almost no evidence and far too much personal abuse. Nearly everyone had some personal story about a member of the other group and how he was 'fooled', if a Psychoanalyst, or 'unaware of clinical material' if a Philosopher. No names were named but it seems so much anecdotal, rather than honest criticism and/or praise. In my opinion there was just too little knowledge of the other's field to make such a book fully rewarding, or such conferences worth-while. Perhaps some preliminary conferences and many years later a book, but I had the feeling of isolation, one from the other. Williams made a pertinent comment on this point, in his discussion of Lazerowitz's paper: (page 165) "His errors, in fact, are just such as we should expect from a person who for years has not looked at any philosophy except from the outside, to get something on it." It was my experience that it is necessary to immerse oneself into any given doctrine (without losing sight of shore naturally) in order to comprehend it, to evaluate it and to put it into proper perspective. Neither group has, generally speaking, tried very hard to immerse themselves in the other's discipline, with the exceptions mentioned, and this makes for relatively dull reading in places.

The only real excitement in the book comes in the occasional struggle, mostly in the first third of the book, as to who shall hold the candle of understanding. Who shall examine and describe the 'orderly' processes of the human mind, the Philosopher who has a long history, or the Psychoanalyst the sixty-year old upstart? There are some real efforts made in this direction, which I do not consider to be either scientific nor in the effort of exploring what is scientific. They are rather social or psychological ploys, much in the nature of 'point of order' to gain the role of the one entitled to judge the proceedings; they add little to the symposium. Yet it is interesting to note that philosophy seems a bit startled by this, here and there throughout the book I expect more sophistication on both sides.

Frankel, a Philosopher, gives a fair estimate of the Freudian position, prior to 1920, in this regard, saying that Psychoanalysis can point only to the negative side of things, and seems unable to explain

reasonable and normal human beings. He does not define the latter term but his point is well made, and has been corrected by Freud himself, since 1920 and many writers since. As such, I think that Psychoanalysis has moved a step closer to a true philosophical position (with inclusion of study of ego processes e.g.).

This is not a first reader in either field and will only confuse and bore a person who tries to so read it this way. For others it might well be worth reading; I would suggest not reading parts but the entirety, in order to get the sense of the on going activity at the conference.

Almost as if to answer some of the comments in Hook's symposium, we have an excellent and easy reading book from the Academy of Psychoanalysis, edited by Jules Masserman. This group, composed of graduate Psychoanalysts, has set out to "extend or rebuild (Freud's) mansions," one step in which direction is taken by this book. Two subjects are examined, one being an outgrowth of the December 1957 meeting, the other from the May 1958 meeting. Each will be discussed separately.

The readers of this Journal will find the first half of the book much more interesting from a theoretical viewpoint. Masochism is discussed and without much, if any, reference to the death instinct. An excellent clinical paper starts the discussion. This lead paper was written by Salzman of Washington, D. C., and in it he tries to describe the complex process that masochism represents in the terms of ego activity. His main theme bears on 'current sacrifices for eventual gains.' The remaining discussions and papers all give slightly different views but all are highly clinical and show a remarkable amount of flexibility. There are papers by Clara Thompson, May Romm, Paul Hoch, John Millet, William Silverberg and a most clever and entertaining summary by Dr. Masserman. This entire section is a must for the well read Psychologist, especially if his work includes any opinions about psychoanalysis.

The second half of the book is entitled 'FAMILIAL AND SOCIAL DYNAMICS' and does not come up to the first half as a scientific effort. While many of the Social and Familial aspects of behavior were discussed, there was much less clinical material and more of what seemed like a striving to prove that the Psychoanalyst should, at least in some cases, come out of his isolation and confer with other members of the family, in order to know what factors come to bear on the patient. I really feel that the general psychological reader will not find this too pertinent. Some interesting work by John Spiegel, on 'Classification of Value Orientations' is summarized here, and there are two papers on various clinical aspects of incest and incest perception. Both of these raise interesting points if these subjects are of particular interest to the reader.

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Arieti, Silvano (Ed.) *American handbook of psychiatry*. New York: Basic Books, 1959.

Even at first glance these two volumes are impressive by virtue of their quantitative characteristics: twenty-five dollar cost, about 2100 double-columned pages (112 of these for author and subject indexes), 100 chapters, 111 authors (a veritable *who is who* in American psychiatry).

Generally, the writing appears to be neither of the depth nor completeness for the professional psychiatrist or research worker but certainly the articles are *by* psychiatrists. Few psychologists (and these mostly from clinical settings) are among the contributors. More distressing are the infrequent references to work of academic psychologists or their journals. The book is no testimonial for psychology as a science basic to the practice of psychiatry. Whether the fault is that of the authors or that of the science might prove a topic for lively debate.

Like other cooperative efforts these pages also suffer from unevenness of treatment and quality; nor are they distinguished by originality of contribution since the viewpoints are expressed elsewhere. Furthermore, we would have to conclude that parts of this work are not written in comprehensive *handbook* style. The quarrel is not with the completeness of the spectrum of titles (this is one of the positive features) or whether various viewpoints, methods, developments are represented in the book. It is rather *within* topics that it fails to meet a criterion of reasonably complete coverage of viewpoint, methods and findings.

These books consist, primarily, of papers, valuable ones, by experts each speaking his views on a particular topic. The collection is like an expanded text or book of "readings" too heavy to carry to class, but particularly valuable for supplemental use because of topics not customarily found in the abnormal psychology text and because of the psychiatric (biocentric or "dynamic") point of view. Following are some of the more unusual topics: statistical data on mental illness; the family of the patient; maladjustments in the aged; a section of five chapters on problems of childhood and adolescence; language and pain; five chapters each on psychoanalytic, non-psychoanalytic, and physical therapies; basic science and experimental psychiatry (six chapters); religion; psychiatry and philosophy; chapters on legal, administrative, didactical and preventative psychiatry, et cetera.

Despite the fact that these volumes are not directed primarily to the psychologist, it would be convenient to own them and certainly they belong on the library reserve shelf for students of abnormal psychology.

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Skinner, B. F. *Verbal Behavior*, New York: Appleton-Century-Crofts, 1957. Pp X+478.

Skinner's *Verbal Behavior* is in many respects unique. Firstly, the author himself is unique; all of us have come to expect certain qualities in his work, and these expectations have not been disappointed. This is a very ambitious and stimulating book, full of examples, insights, and neologisms. It is the first large-scale attempt by a psychologist (with the exception of J. R. Kantor's *Objective Psychology of Grammar*) to account systematically for this uniquely human class of behavior. The aim is achieved with an impressive economy of means; Skinner describes verbal behavior in terms of more general psychological principals without invoking special processes or principles. Still another unique feature of the book is the fact that it represents twenty-five years of research, study, and profound thinking by a single individual. In these days of edited collections of previously published papers and similar ploys of operatorship such works are especially worthy of reinforcement. Unfortunately much serious scholarship is presented in a form which smacks of intellectual snobbery: examples are drawn with a minimum of context from literary works with which every educated person should be, but rarely is, acquainted (Conrad, Butler, Shakespeare, Trollope, and the Bible) or from works known only to specialists (Tooke); phrases in French, Latin, and Greek are used freely—always without translation; and "simple" verbal puzzles (e.g. the impossible one on page 290) are tossed off without a clue to solution. This is not merely a personal expression of annoyance at being made to feel stupid. Skinner's almost exclusive reliance upon literature and everyday life as a source of evidence would never lead one to suspect that psychologists and others have gotten more direct evidence, or even, that they could. This certainly is a disservice to the ideal of objective analysis.

The essentials of Skinner's analysis. The broad outlines of Skinner's analysis of verbal behavior are completely predictable to anyone familiar with his previous verbal behavior. This is a functional analysis (and function here often bears the added meaning of utility for the adaptation of the organism) of observable behavior into response classes and the variables affecting them. The focus is upon behavior rather than upon constructs such as ideas, intentions, meaning (or its modern reincarnation: information) and other ghosts which traditionally haunt this area. Traditional units of analysis such as words, sentences, phonemes, are also rejected in favor of the total speech episode (which is never mentioned after its initial introduction). Ver-

bal behavior is defined as behavior involving speaker and listener in a unique relationship, namely, that the listener must be responding in ways which have been conditioned "precisely in order to reinforce the behavior of the speaker". Subclasses of verbal behavior are distinguished in terms of their controlling variables: conditions of the organism (mands); objects and events or properties of objects and events (tacts); other words (echoic, textual, and intraverbal behavior); the audience (jargon, languages, and special repertoires); other behavior of the speaker (the autoclitic). In actual practice, of course, much verbal behavior is multiply determined.

Each of the functional classes above bears a descriptive role traditionally assigned to a mentalistic processess. The mand, for example, serves the function of speaker's need or intention without necessitating the introduction of such concepts. The mand, as a response class, is reinforced by characteristic consequences and is under the control of motivating conditions and past history of reinforcement with respect to a given audience rather than specified relations to prior stimuli. In other words, it comprises commands, threats, entreaties, requests, etc, all of which are developed as a result of reinforcement of their emission under a particular state of deprivation or aversive stimulation; i.e. you say "water" when thirsty not because you want water but because this utterance has been reinforced with water in the past. Sign, symbol, idea, etc. are replaced with the tact: a verbal operant under the control of objects, events, or properties of objects and events. This takes care of nouns, adjectives, adverbs, declarative statements and concepts as well as metaphor, metonymy and proper names. The little words like "if," "and," "but" which have bedeviled linguistic theorists attempting to derive all language from concept formation (tacting) are accounted for by echoic, textual, intraverbal behavior and by audience control. The linguists' problem is not completely solved, however, because although Skinner accounts for the development of words in an individual's repertoire he does not account for their initial appearance in the repertoire of a language community. Meaning is at least partially accounted for by intraverbal behavior, which term refers to word association as a result of prior reinforced contiguous emission, e.g. 1492: Columbus.

The "speaker", "knower" or directing personality usually assumed to underly the production of speech become gratuitous fictions whose role is assumed by the class of autoclitics which are under the control of preceding speech behavior. The autoclitic is an extremely versatile operant in the ordering and arranging of verbal behavior; it may take the form of description, assertion, qualification, quantification, or a mand upon the listener. Although a heavy descriptive burden is placed upon the autoclitic it is never very clear (to this reader) what the autoclitic is or of what variables it is a function. The autoclitic is

somewhat of a higher-order intraverbal or a tact with respect to one's own verbal behavior. In many instances it serves as a discriminative stimulus for control of the listener's behavior, e.g. the assertion "you are a liar" would bring aversive consequences which are forestalled by an appropriate autoclitic such as "I would be the last person in the world to say that . . ." or "if I didn't know you better I would say that . . .", etc.

The final section of the book is devoted to a detailed consideration of several classes of verbal behavior: thinking, self-editing, and logical and scientific behavior. There are also two personal epilogues (the most enjoyable reading in the book) and an appendix dealing with the development and modification of language within the verbal community.

Final evaluation. After reading *Verbal Behavior* one is enormously impressed with the ambitiousness of the undertaking, the power and parsimony of its tools, and the wealth of stimulating insights, e.g. little paragraphs on unintentional plagiarism, understanding, the poor retention of proper names, and why cats meow "to be let out" (it is an emotional response to the frustrating situation of not being let out which is reinforced by door opening). Moreover, study of the book has led to a richer understanding of the determinants of my own verbal behavior, e.g. my consolation to a colleague who was unsuccessfully attempting to diet that "some people are oral and they just can't lick it". But beneath the awe runs an undercurrent of disappointment whose source is difficult to verbalize. One possible source is the difficulty of the book (my defeat by the autoclitic has already been mentioned). Part of the incomprehensibility, I suspect, stems from a twenty year gestation period during which audience control changed from a small homogeneous group, psychologists, to both a heterogeneous audience containing logicians, linguists, and literary critics and an audience of one: the self-editing behavior of B. F. Skinner. Other doubts arise upon careful examination of Skinner's functional analysis. In doing battle against the demons of meaning, consciousness, and symbol has he found refuge in the Freudian defense of identification with the enemy? Is this brilliantly objective analysis more than the old demons in a new disguise of impeccable language? On some points Skinner's analysis is indistinguishable from Freud's, for all that the censor is not introduced to the audience. All doubts as to the novelty of Skinner's treatment would be dispelled if only he had explained why the particular functional classes were chosen and by what criteria they and no others emerge as basic. In *The Behavior of Organisms* the criterion was explicitly empirical: the

emergence of lawful relationships experimentally demonstrated. In the present case, however, experimental evidence, or even the development of testable implications, is in short supply. Or perhaps I am obtusely refusing to accept the value of post-hoc analysis of passages from *Finnegan's Wake*.

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EDITH D. NEIMARK

English, Horace B. and English, Ava Champney *A comprehensive dictionary of psychological and psychoanalytical terms*. New York: Longmans, Green & Co., 1958. Pp. xiv & 594.

This useful book (modestly sub-titled "a guide to usage") immediately invites comparison to its predecessor: Warren's *Dictionary of Psychology*. In sheer bulk the present work surpasses Warren by 222 pages, and, of course, is 24 years younger. Comparisons of the treatment given to terms which have gained ascendancy in recent years (e.g. "reinforcement") indicate that the Englishs' dictionary (309 lines, 9 definitions, 9 cross references, 23 compounds) will swiftly supplant Warren's (15 lines, 2 definitions, 1 compound) in general use. Two distinctive and extremely useful features which appreciably add to the book's utility are the expanded discussions which frequently follow definitions, and the 272 general articles scattered throughout the work. This reviewer made no attempt to verify the jacket claim that over 13,000 terms are defined, but sees no reason to doubt the statement.

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BOOKS RECEIVED

- THIBAUT, J. W. & KELLEY, H. H. *The social psychology of groups*. New York: Wiley, 1959. Pp. 313.
- JANIS, I. L. et al. *Personality and persuasibility*. New Haven: Yale University Press, 1959. Pp. 333.
- BROWNE, C. G. & COHN, T. S. *The study of leadership*. Danville, Illinois: Interstate, 1958. Pp. 487.
- ABT, L. E. & BELLAK, L. *Projective psychology*. New York: Grove, 1959. Pp. 485.
- GAGNE, R. M. & FLEISHMAN, E. A. *Psychology and human performance*. New York: Holt, 1959. Pp. 493.
- WHITLEY, O. R. *Trumpet call of reformation*. St. Louis: Bethany, 1959. Pp. 252.
- REDHEAD, J. A. *Putting your faith to work*. Nashville: Abingdon Press, 1959. Pp. 128.
- WATTS, A. W. *The way of Zen*. New York: New American Library, 1959. Pp. 224.
- LOVELL, K. *Educational psychology and children*. New York: Philosophical Library, 1959. Pp. 272.
- CHANNELS, V. *The layman builds a Christian home*. St. Louis: Bethany, 1959. Pp. 95.
- GODFREY, E., FIEDLER, F. & HALL, D. *Boards, management, and company success*. Danville, Illinois: Interstate, 1958. Pp. 134.
- ARIETI, S. (Ed.) *American handbook of psychiatry*. New York: Basic Books, 1959. Pp. 2098. 2 vols.
- STRUNK, O. (Ed.) *Readings in the psychology of religion*. Nashville: Abingdon Press, 1959. Pp. 288.
- HOFFMAN, F. J. *Freudianism and the literary mind*. New York: Grove, 1959. Pp. 350.
- STEKEL, W. *Patterns of psychosexual infantilism*. New York: Grove, 1959. Pp. 412.
- RANK, O. *Beyond psychology*. New York: Dover, 1959. Pp. 291.
- ANDERSON, G. C. *Man's right to be human*. New York: William Morrow, 1959. Pp. 191.
- CORLOW, L. & KATKOVSKY, W. (Eds.) *Readings in the psychology of adjustment*. New York: McGraw-Hill, 1959. Pp. 541.
- LEY, W. *Willy Ley's exotic zoology*. New York: Viking, 1959. Pp. 468.
- GALANTER, E. (Ed.) *Automatic teaching: The state of the art*. New York: Wiley, 1959. Pp. 198.
- WINN, R. B. (Ed.) *John Dewey's Dictionary of education*. New York: Philosophical Library, 1959. Pp. 150.
- ROE, ANNE (Ed.) *Graduate education in psychology*. Washington: American Psychological Association, 1959. Pp. 97.
- RUNES, D. D. *Pictorial history of philosophy*. New York: Philosophical Library, 1959. Pp. 406.
- MONTAGU, A. *The cultured man*. New York: PermaBooks, 1959. Pp. 308.
- ATTNEAVE, R. *Applications of information theory to psychology*. New York: Holt, 1959. Pp. 120.
- DIAMOND, S. *Information and error*. New York: Basic Books, 1959. Pp. 307.
- BASS, A. D. (Ed.) *Evolution of nervous control*. Washington: American Association for the Advancement of Science, 1959. Pp. 231.
- ADLER, K. A. & DEUTSCH, D. (Eds.) *Essays in individual psychology*. New York: Grove, 1959. Pp. 480.

- FERENCZI, S. *Sex in psycho-analysis*. (Pp. 288) FERENCZI, S. & RANK, O. *The development of psycho-analysis*. (Pp. 68) New York: Dover, 1959. Bound in one volume.
- COHEN, MABEL B. (Ed.) *Advances in psychiatry*. New York: Norton, 1959. Pp. 314.
- BULLARD, D. M. (Ed.) *Psychoanalysis and psychotherapy (selected papers of Frieda Fromm-Reichmann)*. Chicago: University of Chicago, 1959. Pp. 350.
- WOODWORTH, H. *Sanity, unheard of*. Victoria, B. C.: Sumas, 1958. Pp. 100.
- DANZIG, E. R., THAYER, P. W., & GALANTER, L. R. *The effects of a threatening rumor on a disaster-stricken community*. Washington: National Academy of Sciences—National Research Council, 1959. Pp. 116.
- PERRY, HELEN S. & PERRY S. E. *The schoolhouse disasters*. Washington: National Academy of Sciences—National Research Council, 1959. Pp. 66.
- WRIGHT, BEATRICE A. (Ed.) *Psychology and rehabilitation*. Washington: American Psychological Association, 1959. Pp. 146.
- LANGER, L. *The importance of wearing clothes*. New York: Hastings, 1959. Pp. 349.
- THORNDIKE, R. L. & HAGEN, ELIZABETH. *10,000 careers*. New York: Wiley, 1959. Pp. 346.
- NACHT, S. *Psychoanalysis of today*. (translated by Ruth E. Roman) New York: Grune & Stratton, 1959. Pp. 228.
- RUBENSTEIN, E. A. & PARLOFF, M. B. (Eds.) *Research in psychotherapy*. Washington: American Psychological Association, 1959. Pp. 293.
- BUSH, R. R. & ESTES, W. K. (Eds.) *Studies in mathematical learning theory*. Stanford: Stanford University, 1959. Pp. 432.
- BROWN, W. F. *The girl in the Freudian slip*. New York: New American Library, 1959. Pp. 128.
- REIFF, R. & SCHEERER, M. *Memory and hypnotic age regression*. New York: International Universities Press, 1959. Pp. 253.
- GILL, M. M. & BRENNAN, MARGARET. *Hypnosis and related states*. New York: International Universities Press, 1959. Pp. 405.
- GARNER, ANN M. & WENAR, C. *The mother-child interaction in psychosomatic disorders*. Urbana, Illinois: University of Illinois, 1959. Pp. 290.
- FERGUSON, G. A. *Statistical analysis in psychology and education*. New York: McGraw-Hill, 1959. Pp. 347.
- NUNNALLY, J. C. *Tests and measurements*. New York: McGraw-Hill, 1959. Pp. 446.
- SPIEGEL, E. A. (Ed.) *Progress in neurology and psychiatry*. New York: Grune & Stratton, 1959. Pp. 656.
- KOCH, S. (Ed.) *Psychology: A study of a science. Study 1. Conceptual and systematic. Vol. 2. General systematic formulations, learning, and special processes*. New York: McGraw-Hill, 1959. Pp. 706.
- KOCH, S. (Ed.) *Psychology: A study of a science. Study 1. Conceptual and systematic. Vol. 3. Formulations of the person and the social context*. New York: McGraw-Hill, 1959, Pp. 837.
- BAYER, L. M. & BAYLEY, N. *Growth diagnosis*. Chicago: University of Chicago, 1959. Pp. 241.
- DOLLARD, J. & AULD, F. *Scoring human motives: A manual*. New Haven: Yale University, 1959. Pp. 452.
- ALLEN, R. M. et al. (Eds.) *Proceedings of the postdoctoral workshop in psychological services for the cerebral palsied*. Coral Gables: University of Miami, 1959. Pp. 48.

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